Essays on Macroeconomics, Fiscal Policy and Income Mobility

Mario Alloza Frutos

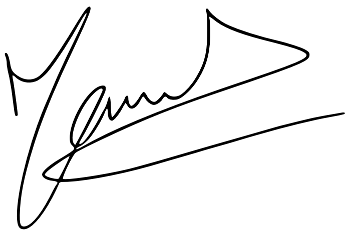
Thesis submitted in fulfilment of the requirements for the degree of Doctor of Philosophy

Department of Economics
University College London

August 2016

Declaration

I, Mario Alloza Frutos, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the work.



Abstract

This dissertation consists of three essays on the effects of fiscal policy on different aspects of the economy. These papers share and empirical nature and exploit both macro and microdata to provide an answer to questions related to the effect of government spending and taxation in the postwar US.

The second chapter analyses the impact of government spending shocks on economic activity during periods of high and low uncertainty and during periods of boom and recession. I identify exogenous government spending shocks using both a structural vector autoregression with exclusion restrictions and narrative methods based on news about future defence spending. I find that government spending shocks have larger impacts on output in booms than in recessions and larger impacts during tranquil times than during uncertain times.

The third chapter investigates how taxes affect relative mobility in the income distribution in the US. I employ household panel data drawn from the PSID between 1967 and 1996 to analyse the relationship between marginal tax rates and the probability of staying in the same income decile. I identify exogenous variation in marginal tax rates by using counterfactual rates based on legislated changes in the tax schedule. I find that higher marginal tax rates reduce income mobility.

The fourth chapter explores the recent trends in intergenerational mobility in the US and how fiscal policy has affected them. I consider the relationship between the level of income of fathers and sons using household data from the PSID. I then investigate how changes in taxation resulting from recent fiscal reforms may have affected such relationship. I find evidence that suggest that sons whose fathers have benefited from fiscal reforms that reduced taxes, are less likely to inherit the income status of their fathers.

Acknowledgements

I am deeply grateful to my supervisor, Morten Ravn. His guidance, patience and kind support have made this thesis possible. He has taught me to critically think about economic issues, to develop new ideas, and to always aim high with my work. His passion for economics and his deep insights have allow me to grow as an economist and enjoy doing so.

I am also indebted to my second supervisor, Raffaella Giacomini, and Vincent Sterk. Their continuous guidance and advice are invaluable resources that have greatly helped me during my PhD studies.

The content of this thesis has also benefited from interacting with many faculty members and fellow PhD students at UCL, particularly those from the Macroeconomics Reading Group. Their advices, comments and suggestions have helped me to improve my research.

My PhD studies have been funded by scholarships from the Bank of Spain and the Department of Economics at UCL. I am most grateful to both institutions for this opportunity.

My parents and, especially, my brother have been an enormous source of encouragement. I owe them much of my achievements in this thesis.

Lastly, and most importantly, I have to thank Alia. She has always supported me, encouraged me and inspired me during the good and bad years of my PhD studies. It is difficult to imagine having finished this enterprise without Alia. This thesis is dedicated to her.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

Do government purchases stimulate the economy? How individuals change their behaviour when facing changes in their tax liabilities? Questions related to fiscal policy have always been central in the study of economics.[1] The importance of this type of policies has increased in the recent years, when monetary policy (the other pillar of government action) has remained constrained.

This dissertation consists of three essays on the effects of fiscal policy on different aspects of the economy. These chapters share and empirical nature and exploit both macro and microdata to provide an answer to questions related to the effect of government spending and taxation in the postwar US.

In the second chapter, titled *Is Fiscal Policy More Effective in Uncertain Times or During Recessions?*, I analyse how uncertainty and the state of the business cycle affect the effectiveness of government spending. There are reasons to believe that the effects of fiscal policy may differ when the economy faces a heightened level of uncertainty or slack (e.g. the presence of adjustment costs suggests that firms will be more cautious and therefore less responsive to stimuli). I attempt to shed light on this question by empirically characterising

[1]See Ramey (2011b) and Keane (2011) for recent surveys on the effects of government spending and taxation.

the response of output and other macroeconomic aggregates to an exogenous shock in government spending that occurs during these states.

I consider the US economy to face a heightened period of uncertainty when the stock market volatility is unusually high. To define periods of recessions, I follow the NBER's dates of business cycles. In order to identify exogenous movements in government spending, I use two of the most used approaches in the literature. The first one is a structural vector autoregression that imposes an exclusion restriction on the contemporaneous response of government spending (justified by the time lag in the response of the fiscal authorities to economic developments). The second method identifies government spending shocks directly by using narrative methods, which consist in quantifying the changes in government defence spending at the moment of their announcement looking at newspapers and other periodicals (see Ramey (2011a)). The results are remarkably similar in both identification strategies and suggest that an exogenous increase in government spending during times of high uncertainty or recession may have contractionary effects on output (while being expansionary during times of booms or low uncertainty). This evidence, which has important policy implications, contrasts with other works that find government spending to be more effective in stimulating the economy during periods of recessions. I reconcile both views and conclude that these differences arise from the information used to define periods of recessions.

The third chapter, entitled *The Impact of Taxes on Income Mobility*, represented my job market paper in 2016. I analyse the effect of tax reforms on the relative movements of households along the distribution of income in the US. The design of the tax schedule has important implications for income inequality, but it may also affect the decisions of households to take advantage of economic opportunities. Following a standard labour supply model, a household experiencing a positive wage shock in a given period may find it optimal to increase its labour supply. This could result in an increase in the relative position in the distribution of income with respect to households that did not experience the same shock. However, high tax progression reduces the households' incentives to take advantage of these opportunities, what would result in fewer movements in the income distribution. To investigate the relationship between taxes and income mobility, I use household panel data from the PSID and construct the marginal tax rate that a married couple faces according to their income, their demographic characteristics and the design of the federal tax schedule of that year.

I estimate the impact of changes in the marginal tax rate on the probability that households stay in the same quantile of the distribution. To address endogeneity issues, I identify exogenous variation in the marginal tax rates by looking at the several legislated tax changes that occurred in the US at the federal level during 1967 and 1996. I find that taxes have a negative and significant effect on income mobility: a percentage point increase in the marginal tax rate reduces the probability that a household changes to a different income decile by almost 1% (which implies that a seven percentage point reduction in the tax rate can explain a tenth of the yearly average movements in the income distribution). This effect, which is found to be robust along several dimensions, has important implications for policies that aim to reduce economic disparities, since tax reforms targeting income inequality are likely to affect income mobility as well. In future research, I intend to study this question further by analysing the dynamics and joint welfare implications of both income inequality and mobility in the framework of a dynamic general equilibrium with ex-post heterogeneity. A model in the spirit of Bewley-Hugget-Aiyagari coupled with labour supply decisions and investment in human capital is able to general income and wealth inequality, because households experience difference wage or productivity shocks that lead them to adjust their labour supply and accumulate their gains or invest them to obtain higher income income the future. This is an adequate framework to understand the welfare effects of changes in income inequality and mobility produced by reforms of a progressive tax schedule.

The fourth chapter, *Do Tax Changes Affect Intergenerational Mobility?* explores how fiscal policy may have contributed to the transmission of income status from parents to children. In the recent years, the distribution of income and the equality of opportunity are topics that have attracted much attention from policy makers (Krueger (2012)). But does fiscal policy increase the degree of social mobility? In this chapter I consider the relationship between the level of income of fathers and sons using household data from the PSID. I then construct a measure of changes in tax liabilities arising from legislated changes in the tax code for each household. With these ingredients I investigate how changes in taxation affect the degree of intergenerational mobility. I find evidence that suggests that sons whose fathers have benefited from fiscal reforms that reduced taxes, are less likely to inherit the income status of their fathers. Particularly I find that the difference in the elasticity between a family that faces a 1,000-dollar change and a family who does not is around 5 percentage points. A potential mechanism that may bring about these results is one based on the decision of parents to invest in the stock of human capital of their children. Reforms that reduce the tax liabilities that parents face, enable them to use these extra resources in funding education and providing their children with better opportunities, what would translate in higher intergenerational mobility. These results suggest that, through income taxation, fiscal policy can impact on the equality of opportunity between generations.

This is an important dimension that policy actions should take into account when considering their medium run effects.

Chapter 2

Is Fiscal Policy More Effective in Uncertain Times or During Recessions?

2.1 Introduction

How do uncertainty and the state of the business cycle affect the effectiveness of fiscal policy? Economic models incorporating non-convex adjustment costs, as in Bloom et al. (2012), suggest that high levels of uncertainty make agents more cautious when taking investment/hiring decisions, thereby reducing the effect of fiscal policy.[1] Michaillat (2014) argues that slackness in the economy will improve the effectiveness of some fiscal policies.[2] In this chapter I attempt

[1]Bloom et al. (2012) develop a model in which uncertainty is time-varying and affects the volatility of technology shocks, and firms are heterogeneous and face non-convex adjustment costs in capital and labour. Fiscal policy is modelled as a wage subsidy. The effect of such a policy is smaller when the policy is implemented at the time uncertainty first hits the economy but slightly larger when the policy is conducted one year later.

[2]Michaillat (2014) considers a New Keynesian model with a search and matching friction where an increase in the size of the public workforce during periods of slack (unemployment increases from 5 to 8%) doubles its effect (as measured by the additional number of workers employed when one more worker is employed in the public sector) compared to that under non-recessionary conditions.

to shed light on this question by empirically characterising how uncertainty and the state of the business cycle influence the effects of government spending.

My empirical strategy is based on a nonlinear structural vector autoregression (SVAR) that allows for differing effects of government spending shocks during times of high (HU) and low (LU) uncertainty, or during times of recession (R) and boom (B). Following Bloom (2009), I identify periods of HU as those with unusually high stock market volatility. I define periods of R and B following the NBER's recording of the dates of business cycles. Exogenous shocks to government spending are identified using two alternative strategies. In the first case, the shocks are identified as the residuals in a SVAR that imposes the exclusion restriction that government spending cannot react within one quarter to shocks to output and tax revenues, as pioneered by Blanchard and Perotti (2002). In the second case, I follow a narrative approach and identify government spending shocks using the news about future defence spending produced by Ramey (2011a). The narratively identified shocks are then classified according to whether they occur during times of HU or LU or, alternatively, during times of R or B. This second framework allows us to address issues as anticipation effects of the shocks, and offers an alternative assessment of the exogeneity of the shocks. I apply this methodology to US data between 1948 Q1 and 2007 Q4.

The results suggest that the response of output to a positive government shock is negative during times of HU or R and positive during times of LU or B. Interestingly, the two identification strategies achieve very similar results. These results can be understood in the light of a framework where information is scarce or noisy during times of HU. In this context, agents are concerned that the economy may take a downturn and reduce their future levels of income. A government spending shock during times of heightened uncertainty may then simply confirm these pessimistic views, in turn producing a decline in consumption and activity. I find evidence of measures of household-sector confidence reacting negatively to a government spending shock during times of HU, together with consumption and prices.

The results I obtain contrast with previous literature that finds government spending shocks to be more effective in stimulating the economy during periods of R than B (Auerbach and Gorodnichenko (2012)). I reconcile the two views and conclude that these differences arise from the information used to define periods of R.

By using the VAR framework to obtain impulse response functions, we are imposing the restriction that responses are fixed for each regime (history-independence). I check whether this is an issue using the local projections of Jordà (2005), as suggested in Auerbach and Gorodnichenko (2013) and Ramey and Zubairy (2014).

Traditional empirical research on fiscal policy, starting with the influential work of Blanchard and Perotti (2002) and subsequent papers such as Ramey (2011a) and Barro and Redlick (2011),[3] has focused on the linear effects of fiscal policy (i.e. the effect of the fiscal policy is assumed to be the same regardless of potentially changing conditions). The conclusion of the above research is that government spending stimulates economic activity, although the precise impact, as measured by the so-called fiscal multiplier, is still controversial (Hall (2010)).[4] Another strand of the literature suggests the opposite effect: government spending cuts have expansionary effects under certain conditions. This is the implication of work by Giavazzi and Pagano (1990) and Alesina

[3]Other works on the economic effects of government spending include, for example, Ramey and Shapiro (1998), Burnside et al. (2004), Perotti (2004) and Mountford and Uhlig (2009).

[4]The government spending multiplier is defined as the ratio of output change to an exogenous discretionary increase in government spending. See Ramey (2011b) for a survey on the fiscal multiplier.

and Ardagna (2013).[5]

There is, however, a recent emphasis on allowing for nonlinear effects of fiscal policy, as highlighted in Parker (2011). Corsetti et al. (2013) suggest that the health of public finances might not only affect the magnitude but also the sign of the response of output to government spending. In recessions in an economy with a high level of debt and where monetary policy is constrained (e.g. because of the zero lower bound), an increase in government spending may increase the probability of default, lowering demand. Under certain conditions, the multiplier can shift from positive to zero, or even become negative and large.[6]

Bertola and Drazen (1993) and Bi et al. (2013) argue that expectations about future government spending can also generate nonlinear effects. These authors explore the idea that cuts in government spending can cause an economic expansion if they induce agents to believe that government spending will be higher in the future. Bi et al. (2013) build on this idea and suggest that changes in agents' expectations about fiscal policy (the timing of it and instruments used) can generate positive or negative effects on economic activity, depending on other elements of the economy such as the monetary policy stance or the level of government debt.

A growing body of evidence (Bloom (2009), Baker et al. (2013)) suggests that uncertainty does have a negative effect on economic activity. However, no research so far provides empirical evidence on how uncertainty affects fiscal

[5]See Alesina (2010) for a review of the expansionary effects of fiscal consolidations.

[6]An increasing number of studies are also investigating whether the government spending multiplier can be higher during times when the zero lower bound on nominal interest rates binds. Christiano et al. (2011) argue that large shocks to preferences regarding intertemporal substitution can lead to liquidity traps. In such cases, government spending, by causing inflation, will stimulate output by a much bigger magnitude than during normal times (impact multiplier of 1.6; Fernández-Villaverde et al. (2012) find an impact multiplier of 3 in a nonlinear setting). However, Mertens and Ravn (2014) argue that this effect could be the opposite if we consider that the liquidity trap is caused by an exogenous (sunspot) shock to confidence that drives a shift from optimism to pessimism.

policy.[7] This question could have important implications from a policy-making standpoint, regarding the extent to which a fiscal intervention may be appropriate during a period of turmoil.

My work does relate to an increasing amount of empirical studies focusing on whether business cycle conditions are associated with nonlinear effects of fiscal policy, for example Auerbach and Gorodnichenko (2012), Auerbach and Gorodnichenko (2013), Bachmann and Sims (2012), Mittnik and Semmler (2012), Fazzari et al. (2012), Bognanni (2012), Owyang et al. (2013), Ramey and Zubairy (2014) and Caggiano et al. (2015).[8] However, the variety of methodologies employed and the heterogeneity in the definitions of what can be considered a recession (or a slack economy) yield very different results. Some of these studies find that recessions or periods of slack in the economy make government spending a particularly powerful tool. This is true of Auerbach and Gorodnichenko (2012), one of the most prominent studies in this body of literature. These authors use a smooth-transition SVAR in which the probability of recession is weighted by a seven-period centred moving average of the growth rate of output, their measure of the state of the business cycle. Bognanni (2012) finds the opposite: a smaller multiplier during recessions in a Markov-switching VAR in which the probability of recession is estimated period by period. Owyang et al. (2013) and Ramey and Zubairy (2014), meanwhile, find no impact of the state of the business cycle on government spending multipliers.

The present analysis differs from the studies just cited in two important dimensions. First, I use a simple and transparent methodology that allows

[7]Aastveit et al. (2013) investigate the effects of uncertainty on the effectiveness of monetary policy.

[8]Brückner and Tuladhar (2013) explore the effect of fiscal policy during times of financial crisis. The authors find that firms' financial distress (as measured by a reduction in their net worth because of lower commercial land prices) implies a significantly lower government spending multiplier.

estimation by OLS and the implementation of different identification strategies for government spending shocks. Second, instead of estimating the probability of recession, or using other variables as ways to estimate the output gap, I employ the definition the NBER uses to measure recession.

The rest of the chapter is organised as follows. Section 2.2 describes the empirical strategy and presents the results obtained from the different methods employed to identify the government spending shocks. Since the findings of this chapter are in striking contrast to previous conclusions in the literature, I investigate the sources of these differences. Section 4.5 contains different robustness tests for the results of the benchmark specifications. Section 3.7 concludes and offers directions for future research.

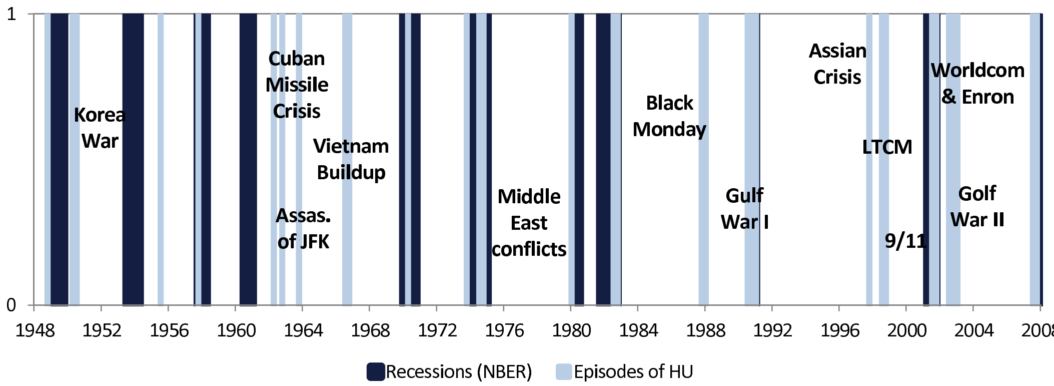
2.2 Empirical Strategy

The empirical literature on the effects of fiscal policy disagrees on which is the best way to identify government spending shocks. The two most commonly used frameworks differ in their assumptions and in the results obtained (see Hall (2010)).[9] The Blanchard and Perotti (2002) methodology makes use of an SVAR and identifies exogenous government spending shocks as the only ones that can affect government spending contemporaneously. The second method identifies government spending shocks following Ramey and Shapiro (1998), who use unexpected changes in defence spending. While recognising that both frameworks have their merits, here I do not take a stance on the suitability of each one but instead use both. They will be described in more detail below.

To define periods of HU I follow the methodology and data described in Bloom (2009). Bloom (2009) constructs a monthly measure of uncertainty

[9] An alternative method would be to restrict the sign of some responses of the system to achieve identification, as in Mountford and Uhlig (2009).

Figure 2.1: Periods of HU and R between 1948 Q1 and 2007 Q4



using the VXO index of implied volatility from 1986 onwards and using the actual monthly return volatilities of the SP500 index between 1962 and 1986.[10] I extend these estimates back to 1948. Major uncertainty events are selected as those months which have a stock market volatility of 1.65 standard deviations above a Hodrick-Prescott trend (with a smoothing coefficient of 129,600). Since the sample has a quarterly frequency, I consider periods of HU to be those quarters containing any of the monthly events described above.[11] Periods of LU are defined as the rest of the quarters.

The definition of quarters of R or B is done by following the business cycle dates produced by the NBER. Figure 2.1 shows the quarters of HU (32 in total) and those of R (35).[12]

For both specifications the data used contains real federal government spending, output and tax revenues in *per capita* terms as described in the Appendix. The sample starts in 1948q1 and finishes in 2007q4.

[10]The adequacy of stock market volatility as a measure of uncertainty is also documented in Bloom et al. (2007).

[11]The results are very similar when I consider quarterly volatility (instead of monthly) and pick up the periods with unusually high values.

[12]See Bloom (2009) for a complete characterisation and description of the HU events since 1962.

2.2.1 The SVAR Approach

To capture the potentially different contemporaneous and dynamic responses of the variables to government spending shocks, I estimate an otherwise standard SVAR with dummy variables that provide information about the change in economic conditions (from times of LU to HU or between R and B):

$$\boldsymbol{x}_t = \boldsymbol{B}_L(L)\boldsymbol{x}_{t-1} + \left(\boldsymbol{B}_H(L) - \boldsymbol{B}_L(L)\right) H_t \boldsymbol{x}_{t-1} + \boldsymbol{e}_t \tag{2.1}$$

$$\boldsymbol{e}_t = \boldsymbol{D}_t \boldsymbol{\varepsilon}_t \tag{2.2}$$

$$\boldsymbol{D}_t = (\boldsymbol{D}_L + \boldsymbol{D}_H H_t) \tag{2.3}$$

where $\boldsymbol{x}_t = [g_t, y_t, tr_t]'$ and $\boldsymbol{e}_t \sim \mathcal{N}(0, \boldsymbol{D}_t\boldsymbol{D}_t')$ is a vector of residuals which are linear combinations of the structural shocks $\boldsymbol{\varepsilon}_t \sim \mathcal{N}(0, \boldsymbol{I})$. $\boldsymbol{B}(L) = (\boldsymbol{I} - \boldsymbol{B}_1 L - \boldsymbol{B}_2 L^2 \ldots \boldsymbol{B}_p L^p)$ represents a lag polynomial of order p.[13]

H_t is a dummy variable that takes a value of one during periods of HU (or R, depending on the analysis).[14] When $H_t = 0$, the dynamic lagged variables affect the system through $\boldsymbol{B}_L(L)$, and when $H_t = 1$ through $\boldsymbol{B}_H(L)$, allowing for a potentially different dynamic response in the system. The contemporaneous response matrix $\boldsymbol{D}_t$ is also allowed to be state-dependent, changing during periods of LU or B (matrix $\boldsymbol{D}_L$) and periods of HU or R (matrix $\boldsymbol{D}_H$). The specification also includes a state-varying constant and a quadratic trend (as emphasised in Francis and Ramey (2009)).

In the framework of this subsection, exogenous shocks to government spending are identified using an exclusion restriction: government spending does not react contemporaneously to other structural shocks. This assumption implies

[13] I set $p = 4$ following Blanchard and Perotti (2002) and Ramey (2011a).

[14] Some studies (see Auerbach and Gorodnichenko (2012)) advocate using H_{t-1} instead of H_t to avoid contemporaneous feedback from fiscal policies into the state of the economy. The results are similar regardless of which specification I use.

that there is a time lag of one quarter required to enact public spending bills. Following Blanchard and Perotti (2002), the plausibility of this restriction rests on the minimum required time that the fiscal authority faces when adjusting government spending to surprise changes in fiscal (as measured by shocks to tax revenues) or general (as measured by shocks to output) macroeconomic conditions. To implement this restriction, the matrix $\boldsymbol{D}_t$ is obtained from a Choleski decomposition of the variance-covariance matrix of the relevant residuals from equation 2.1, where government spending is ordered first.

To prevent the nonlinearities that are present in equation 2.1 from altering the original Blanchard-Perotti identification assumption, I impose 0 coefficients on the matrix $\boldsymbol{B}_1(L) = \boldsymbol{B}_H(L) - \boldsymbol{B}_L(L)$ for the government equation. Therefore, government spending shocks ε_t^g are identified, in line with Blanchard and Perotti (2002), from:

$$g_t = \sum_{j=1}^{p} \boldsymbol{\beta}_{0,j}^{g} \boldsymbol{x}_{t-j} + \varepsilon_t^g$$

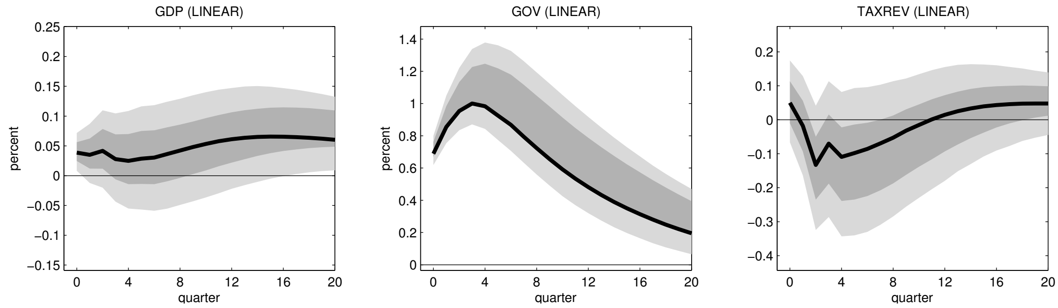
When we do not allow for differential responses due to changing economic distinctions, i.e. $H_t = 0$ for all periods, equation 2.1 reduces to a standard linear SVAR model:

$$\boldsymbol{x}_t = \boldsymbol{B}(L)\boldsymbol{x}_{t-1} + \boldsymbol{D}\boldsymbol{\varepsilon}_t \tag{2.4}$$

I start by estimating equation 2.4 to establish a comparison with previous work. Figure 2.2 shows the responses of the variables in the system to a positive shock in government spending that raises government spending by 1% at its peak. The figure also shows 68% and 95% confidence intervals, computed using a non-parametric bootstrap method.[15] The response of output

[15]To compute the confidence intervals, I generate 100,000 bootstrap draws of the endogenous variables using the estimated coefficients ($\hat{B}_H(L)$ and $\hat{B}_L(L)$) and residuals ($\hat{e}_t$) from equation 2.1. I re-estimate the VAR using the bootstrap samples and compute the 68th and 95th percentiles of the resulting impulse responses.

Figure 2.2: Responses in the linear model (SVAR identification)



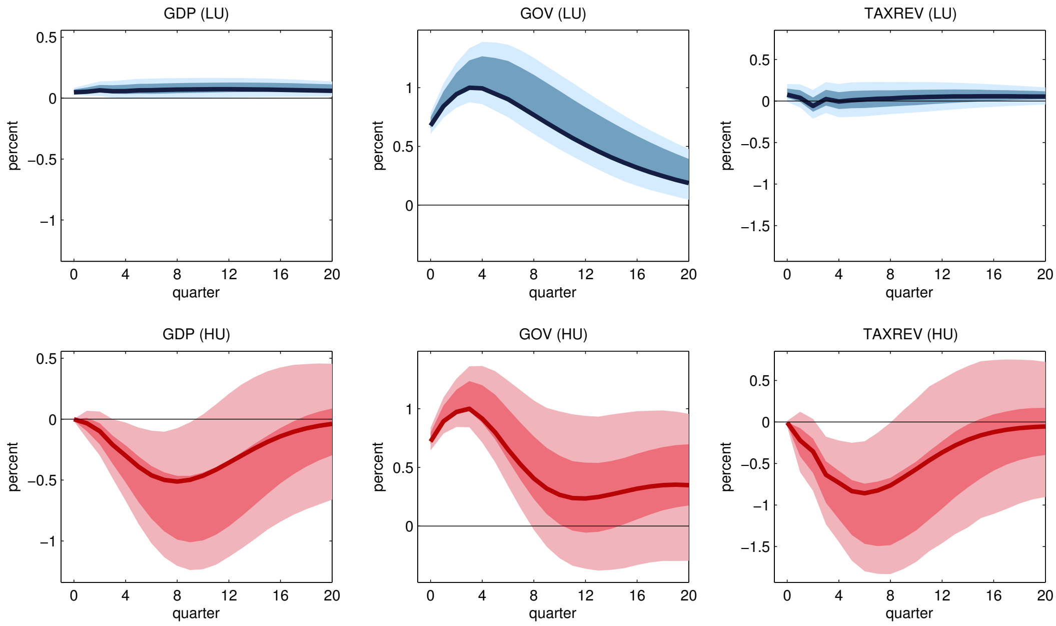
Responses to a government spending shock (identified with exclusion restrictions) in a linear model. 68% and 95% confidence bands computed with a non-parametric bootstrap.

is positive throughout the horizon (20 quarters). The implied elasticity of the GDP on impact with respect to the government spending peak is 0.045. When translated into multiplier terms, the impact multiplier is 0.46, increasing to 0.85 at the peak.[16] The results are qualitatively similar (although slightly smaller in size) to Blanchard and Perotti (2002), with output reaching its peak impact after the fourth year).

I now relax the implied assumption that changes in government spending always have the same effect. When allowing for nonlinearities as described in equation 2.1, the results change dramatically. I start with the case where H_t takes a value of 1 during periods of HU as described above. Figure 2.3 shows the responses to a positive shock to government spending. The first panel (in blue) shows the responses of the variables in the system during times of LU. The response of output is slightly higher than in Figure 2.2 (0.56 on impact, implying an impact multiplier of almost 0.6, and about 1 at the peak) and remains significant throughout the 20 quarters considered. The responses of government spending and tax revenues are qualitatively similar to the linear case. The bottom panel (in red) of Figure 2.3 shows the response to the same

[16]The ratio of nominal GDP to nominal federal government spending in the sample is 10.13.

Figure 2.3: Responses during times of LU and HU (SVAR identification)

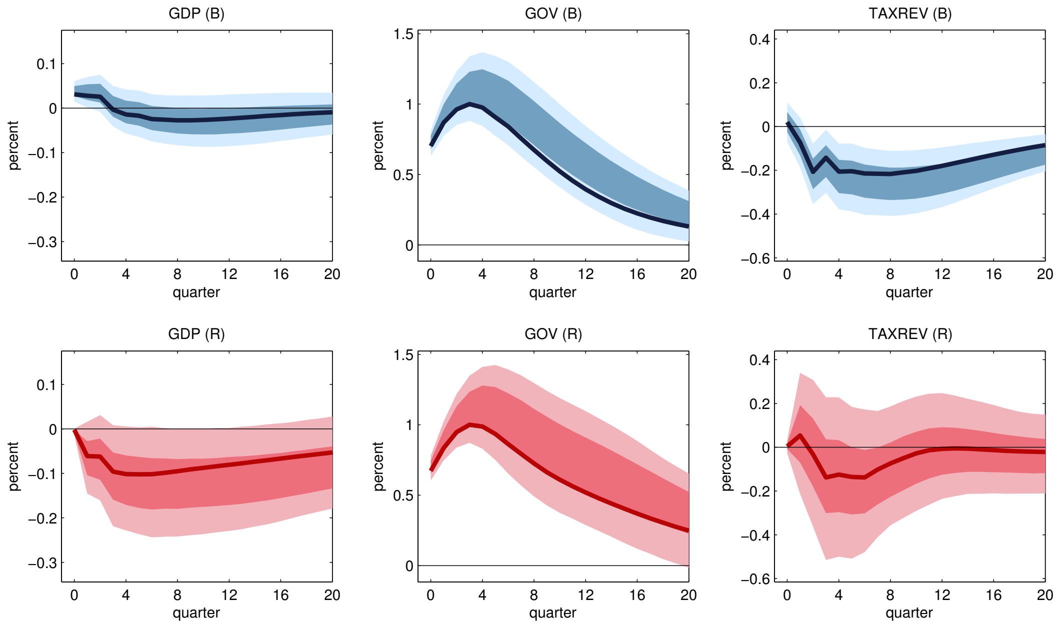


The top panel (in blue) shows responses to a government spending shock (identified with exclusion restrictions) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

shock during times of HU. In striking contrast with the previous case, a positive government spending shock is associated with a large, significant and negative response of output. The effect of the shock is small on impact but it builds up progressively until after the second year, with an elasticity of -0.48 (which in this sample would imply a peak multiplier of -4.9). The response of government spending during times of HU has a similar shape to the linear case during the first three years, although it exhibits more persistent effects from the shock.

Next, I consider the case of nonlinearities caused by the state of the business cycle (H_t takes a value of 1 during periods of R). The responses are shown in Figure 2.4. During times of B, the response of output is positive and very similar to the linear case during the first three quarters, although the shock now has very temporal effects and output becomes slightly negative but insignificant

Figure 2.4: Responses during times of B and R (SVAR identification)



The top panel (in blue) shows responses to a government spending shock (identified with exclusion restrictions) during times of boom. The bottom panel (in red) shows responses during times of recession. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

during the rest of the horizon.[17] During times of R, the response of output to a positive increase in government spending is instead estimated to be negative. The magnitude of this effect is, however, smaller than in the case of the HU periods considered earlier (the elasticity at the peak is -0.1).

These results suggest that the response of output to government spending shocks does not remain the same across all states of the economy. In particular, it becomes negative when we consider periods of HU or R.

2.2.2 The Narrative Approach

In this subsection, I use a different framework to achieve identification of the government spending shocks while retaining the potentially different effects of

[17]Note that this is not the case when we consider a stochastic trend, as in the appendix.

these on the variables in the system.

The specification described in equation 2.1 relies on two assumptions: (i) government spending shocks are a surprise to agents and (ii) government spending cannot react within one quarter to other shocks affecting the economy. However, it could be the case that government spending plans are anticipated by agents, which would violate assumption (i) above. This possible mistiming of events has been voiced as a criticism of SVAR approaches such as that expressed by equation 2.1 (Ramey (2011a)). To avoid this potential issue, I use the measure of news about future government spending (as a percentage of GDP) described in Ramey (2011a) to identify exogenous shocks.[18]

Regarding the second assumption, it could be argued that the intervention lag of one quarter taken by the fiscal authorities to respond to developments in the economy, assumed in the previous subsection, is more likely to be violated during times of R or HU (since it could be the case that governments will act faster in passing bills in such times). This would cause a problem of a lack of exogeneity. The use of narrative identification of shocks using news about defence spending again allows us to deal with this problem, since the defence news variable is more likely to be driven by exogenous foreign political events, wars, etc (Ramey (2011a)).

I now estimate a VAR that explicitly incorporates the structural shocks to government spending, namely ε_t^{Ramey}, or news about defence spending:

$$\boldsymbol{x}_t = \boldsymbol{B}(L)\boldsymbol{x}_{t-1} + \boldsymbol{C}(L)H_t\varepsilon_t^{Ramey} + \boldsymbol{D}(L)(1-H_t)\varepsilon_t^{Ramey} + \boldsymbol{\xi}_t \tag{2.5}$$

[18]Ramey (2011a) constructs a time series of the expected discounted values of government spending changes by obtaining quantitative information about estimated defence spending from periodicals (hence its name of narrative identification). A simpler approach based on the same strategy can be found in Ramey and Shapiro (1998), where the authors use war dates to identify exogenous changes in defence spending.

As before, $\boldsymbol{B}(L)$ is a lag polynomial of order p and $\boldsymbol{C}(L)$ and $\boldsymbol{D}(L)$ are lag polynomials of order q.[19] $\boldsymbol{\xi}_t$ is a residual with normal distribution. As in equation 2.1, the above model allows for government spending shocks to have differential effects, both dynamically and on impact, depending on the evolution of features of the economy controlled by H_t.[20] However, the key difference from the model in equation 2.1 is that the structural shocks ε_t^{Ramey} are now assumed to be observable variables.[21]

When we exclude differential effects of government spending shocks due to economic conditions (i.e. $H_t = 0$ for all periods), equation 2.5 reduces to the standard linear case considered in Ramey (2011a):

$$\boldsymbol{x}_t = \boldsymbol{B}(L)\boldsymbol{x}_{t-1} + \boldsymbol{F}(L)\varepsilon_t^{Ramey} + \boldsymbol{\xi}_t \tag{2.6}$$

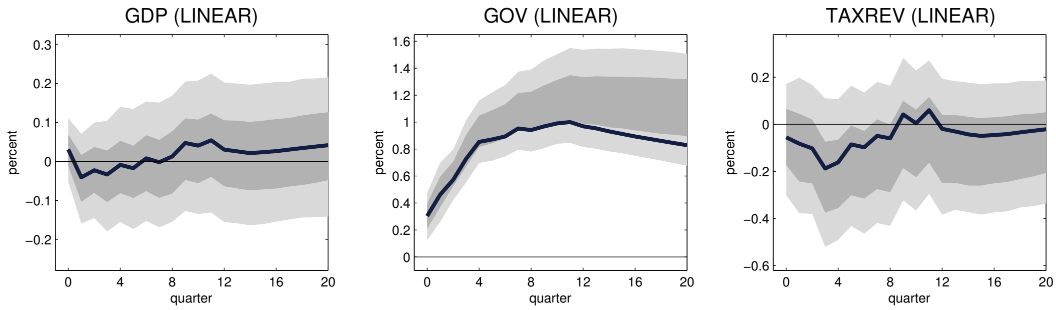
To compare these results to others in the literature, I start by estimating equation 2.6. Figure 2.5 shows the responses of the variables in the system to a positive shock in government spending that raises government spending by 1% at its peak. As before, the figure also shows the 68% and 95% confidence intervals, computed using a non-parametric bootstrap method. The response of output peaks after the second year with an elasticity of 0.47 (multiplier of 0.52), although it is mostly insignificantly different from 0 throughout the horizon (20 quarters). Ramey (2011a) finds a positive and significant response of output when including data from WWII (and other controls, such as the interest rate), although she also finds a similar response to the one found here when excluding both WWII and the Korean War. She concludes that the

[19]Following similar studies such as Romer and Romer (2010), I set $q = 12$.

[20]When H_t takes a value of 1, the contemporaneous and dynamic effects of the shock ε_t^{Ramey} are given by the matrix $\boldsymbol{C}(L)$. Conversely, when $H_t = 0$ these effects are controlled by the matrix $\boldsymbol{D}(L)$.

[21]Section 4.5 relaxes the assumption that the structural shocks ε_t^{Ramey} are perfectly observable and considers the case of shocks measured with error.

Figure 2.5: Responses in the linear model (narrative identification)

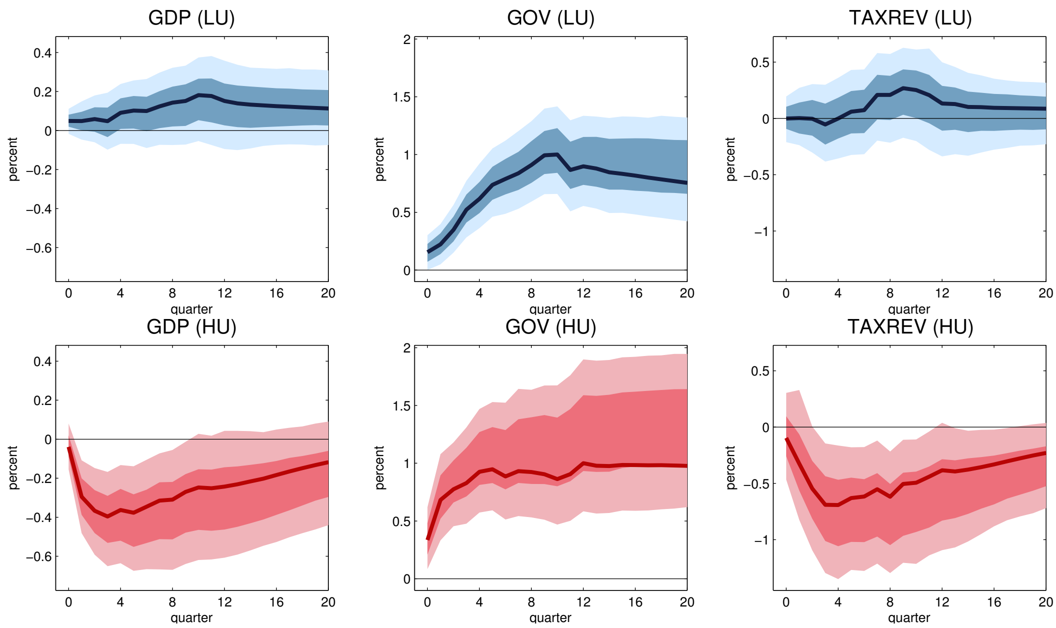


Responses to a government spending shock (identified from narrative accounts of defence spending) in a linear model. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

variable of defence shocks is not informative enough for the post-Korean-War sample.

The results change dramatically when we allow for nonlinear effects as considered in equation 2.5. Figure 2.6 show the responses to a positive shock to government spending for the case where H_t takes a value of 1 during periods of HU as described above. Interestingly, responses of output during both states (LU in the top panel in blue, and HU in the bottom panel in red) are now mostly significant. The signs of the responses follow the same pattern as in the previous subsection, with output reacting positively to a government spending shock during LU, and negatively during HU. In periods of LU, following a positive shock to government spending, output grows by 0.049 and peaks together with government spending before the second year, with an implied elasticity of 0.18 (equivalent to a multiplier of 0.54 on impact and one of about 2 at the peak). Despite the difference between the two identification approaches, the responses during HU are very similar to those obtained using the Blanchard-Perotti restrictions. Output falls by almost -0.04 on impact and reaches -0.4 at the end of the first year (which implies an impact multiplier of -0.4, and

Figure 2.6: Responses during times of LU and HU (narrative identification)



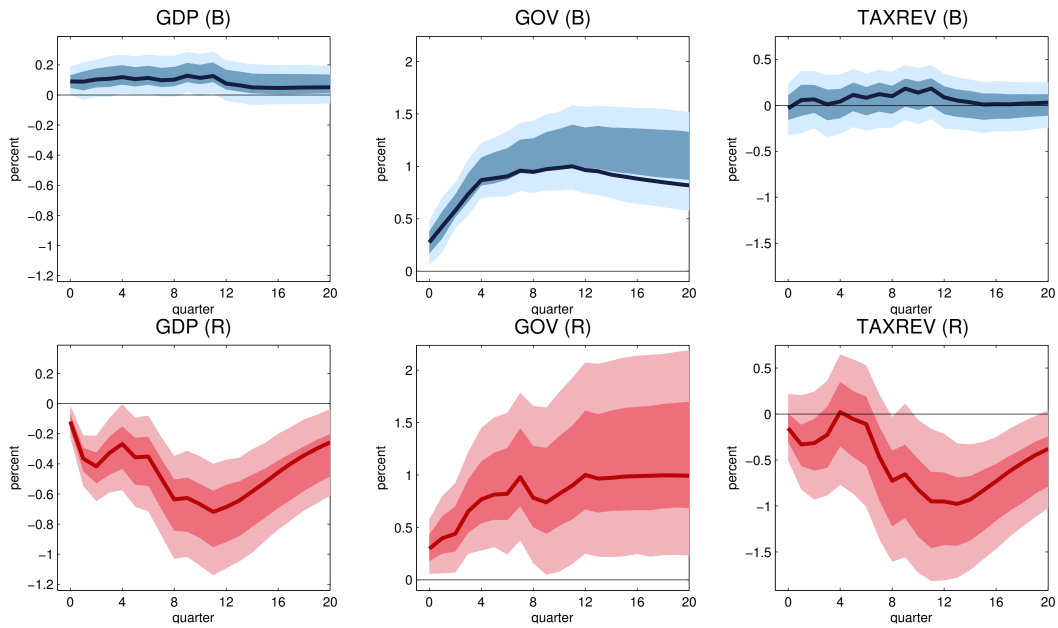
The top panel (in blue) shows responses to a government spending shock (identified from narrative accounts of defence spending) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

-4.35 at the peak).

Surprisingly, while the linear estimation (Figure 2.5) showed little response of output to a government spending shock, the nonlinear estimation uncovers a very different scenario: output reacts positively (and strongly) during times of LU and negatively during times of HU (in a magnitude very similar to that found in the previous identification approach).

Figure 2.7 shows the responses when H_t takes a value of 1 during periods of R. Again, the response of output is significantly different from 0 at most forecast horizons (as opposed to in the linear case). Output reacts positively to a government spending shock during times of B: it peaks at 0.13 together with government spending at the end of the third year, implying a multiplier of 1.4. The response of output is negative when the shock happens during a

Figure 2.7: Responses during times of B and R (narrative identification)



The top panel (in blue) shows responses to a government spending shock (identified from narrative accounts of defence spending) during times of boom. The bottom panel (in red) shows responses during times of recession. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

time of R, although in this case the response is noticeably larger in magnitude (elasticity of -0.7). The responses of government spending, as in the case of HU and LU, are persistent.

The narrative identification approach corroborates the results obtained in the previous subsection: the response of output to a government spending shock depends on the evolution of features of the economy such as the level of uncertainty or the state of the business cycle. The nonlinear estimation also offers a different view to that of the linear case when using the Ramey (2011a) narrative variable, showing that government spending is in fact a powerful instrument in stimulating output (during times of B or LU).

2.2.3 Understanding the Results

Why do high levels of uncertainty or a recessionary economy affect the impact of exogenous changes in government spending? The above analysis suggests that there is a mechanism that operates differently when the economy is in a state of HU (or R). In this subsection, I consider the role of a change in confidence as the device inducing the differing responses in the empirical analysis.[22]

I interpret a shift from times of LU to HU as a deterioration in the information set available to agents.[23] In a context of scarce information, households may become more cautious, rendering their confidence sensitive to signals that may confirm their pessimism about their future income levels.

In such a situation, an increase in government spending could serve to corroborate the idea that the productivity of the economy was low, triggering a shift to pessimism among households. The likely result of the deterioration in households' confidence would be that consumption would decrease in view of potentially low levels of income. Firms would respond to this decrease in demand by lowering production and prices, which would have a contractionary effect on the overall economy.

To support this conjecture, I analyse the responses of some relevant variables to a government spending shock.[24] Figure 2.8 shows the responses of

[22]I focus on a mechanism that acts differently during times of HU and times of LU (rather than during times of B and times of R). Although uncertainty could be endogenously generated during R (see Bloom (2014) for a discussion), the definition of periods of HU here is mostly based on exogenous events (Bloom (2009)), which makes uncertainty a better candidate with which to explain the above results. Since periods of HU and R do not always overlap, we could test which is the ultimate driving force behind the differing responses observed above (heightened uncertainty or a slack economy). Unfortunately, the data are too scarce for us to draw conclusive results on this.

[23]This can be due to scarce information or a reduction in its accuracy. As defined by Frank Knight (1921), uncertainty is found in situations where agents cannot attach probability distributions to some events. This represents the inability of agents to form accurate predictions about, for example, the level of productivity in the economy or the income levels expected by households.

[24]I use narrative identification based on Ramey's news about defence spending. Following Burnside et al. (2004) and Ramey (2011a), I use the fixed set of variables $\boldsymbol{x}_t$ described above

three measures of confidence to a government spending shock during times of LU and HU. The first two columns correspond to the Consumer Confidence Index (CCI) and the Index of Consumer Sentiment (ICS), two popular measures of households' confidence.[25] Both variables react positively to a government spending shock when it takes place during a time of LU, but decrease when shocks occur during times of HU (the CCI decreases markedly and significantly while the ICS does so in a less significant manner). The third column of Figure 2.8 plots the response of an indicator of industrial confidence in the manufacturing sector (the Business Conditions Indicator from the OECD), which behaves similarly. This evidence suggests that government spending shocks lower the confidence of agents if they occur during times of heightened uncertainty, while they boost their confidence during normal times.

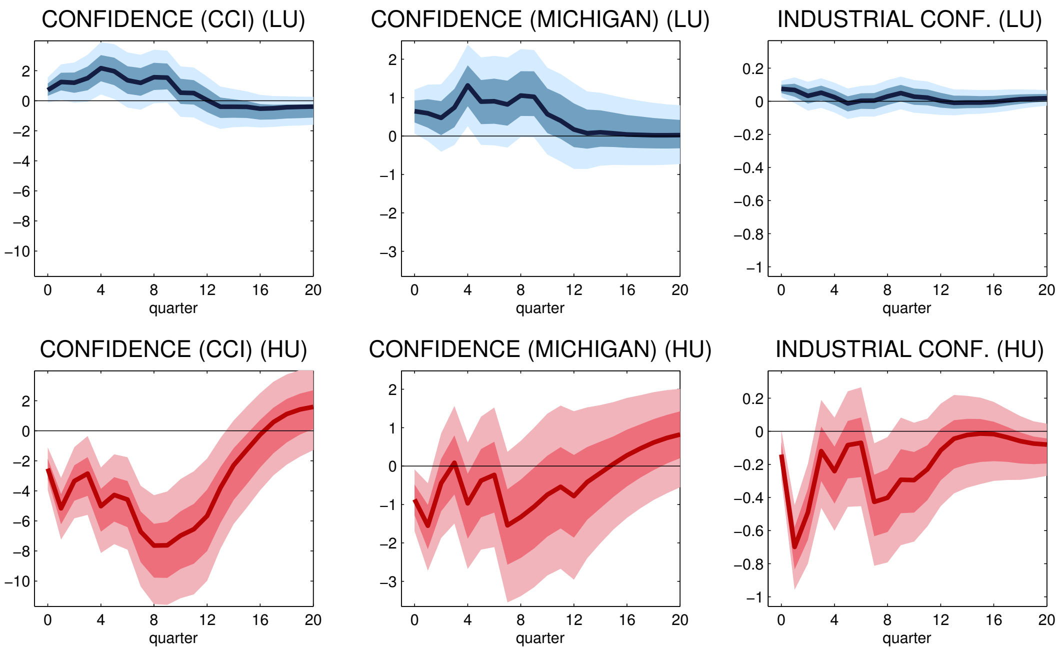
This shift towards pessimism translates into a higher demand for precautionary savings by households. Figure 2.9 shows a significant reduction in consumption in response to government spending shocks during times of HU. In such a scenario, firms would optimally respond to declining demand by lowering prices. This is consistent with the second column of Figure 2.9, which shows that actual (dashed line) and expected (solid line) inflation do not react significantly to the shock during LU, but they decline when the shock occurs during a time of HU.[26] The last column of Figure 2.9 shows the response of

and rotate the new variables of interest into the analysis.

[25]The CCI measures consumer confidence by using the monthly responses of 5,000 US households to questions on their current and expected (within the next six months) business, family income and employment conditions. The CCI is computed as the proportion of participants that respond positively to these questions. Data for this variable is available from 1967 Q1 onwards. Similarly, the ICS is computed by combining the proportions of interviewed people who express favourable opinions on their current and expected (within the next twelve months) financial situation and the business conditions in the country, on their expectations for the next five years about the economic situation, and about their purchases of durable goods in the current period. The ICS uses responses from 500 telephone interviews and is available from 1960 Q1 onwards.

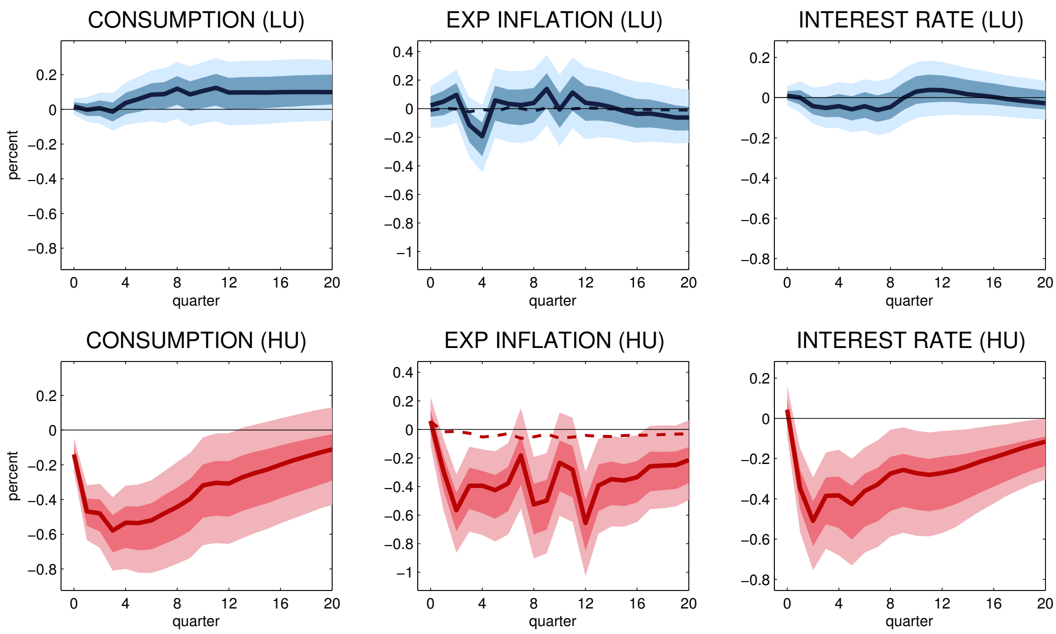
[26]Expected inflation is measured by the median inflation forecast over the next 12 months from the Survey of Consumers (University of Michigan). The results are qualitatively similar when I use inflation forecasts from the Survey of Professional Forecasters.

Figure 2.8: Measures of confidence during times of HU and LU (narrative identification)



The top panel (in blue) shows the responses of different measures of consumers' and firms' confidence to a government spending shock (identified from narrative accounts of defence spending) during times of low uncertainty. The measures of confidence include the Consumer Confidence Index (CCI) provided by the Conference Board (data start in 1967 Q1), the Index of Consumer Sentiment provided by the University of Michigan (data start in 1960 Q1) and the Business Conditions Indicator from the OECD (data start in 1950 Q1). The bottom panel (in red) shows responses during times of high uncertainty. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

Figure 2.9: Further responses during times of HU and LU (narrative identification)



The top panel (in blue) shows the responses of different measures of consumers' confidence to a government spending shock (identified from narrative accounts of defence spending) during times of low uncertainty. The broken lines represent realised inflation, as measured by the Consumer Price Index. Expected inflation is measured by the median inflation forecast over the next 12 months from the Survey of Consumers (University of Michigan). Interest rates are measured using the three-month Treasury bill. The bottom panel (in red) shows the responses during times of high uncertainty. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

interest rates to a government spending shock. As could be expected from the response of the monetary authority to developments in inflation, the interest rate declines after a shock during a time of HU (and remains roughly constant after a shock that occurs in a time of LU).

2.2.4 Relation to Other Studies

The empirical results in this chapter suggest that government spending shocks have negative effects on output during recessions. Other studies arrive at the opposite conclusion: recessions make government spending more expansionary than booms. In order to understand why these findings are so different, I now compare these results with those of Auerbach and Gorodnichenko (2012), whose study is one of the most prominent in this area.[27]

Auerbach and Gorodnichenko (2012) use a Smooth-Transition VAR to investigate the variation in the response of output between periods of R and B. They estimate the following model:

$$\boldsymbol{x}_t = \left(1 - H_{t-1}^{AG}\right) \boldsymbol{C}_B(L)\boldsymbol{x}_{t-1} + H_{t-1}^{AG}\boldsymbol{C}_R\boldsymbol{x}_{t-1} + \boldsymbol{e}_t \tag{2.7}$$

$$H_t^{AG} = \frac{exp(-\gamma z_t)}{1 + exp(-\gamma z_t)} \tag{2.8}$$

$$\boldsymbol{e}_t \sim \mathcal{N}\left(0, \boldsymbol{\Omega}_B \left(1 - H_{t-1}^{AG}\right) + \boldsymbol{\Omega}_R \left(H_{t-1}^{AG}\right)\right) \tag{2.9}$$

$$var(z_t) = 1 \ , \ E(z_t) = 0 \tag{2.10}$$

where $\boldsymbol{x}_t$ is the same vector of variables as defined above. The model allows for a differential impact of the government spending shock both contemporaneously (through matrices $\boldsymbol{\Omega}_B$ and $\boldsymbol{\Omega}_R$) and dynamically (through matrices $\boldsymbol{C}_B(L)$ and $\boldsymbol{C}_R(L)$) during booms and recessions. The transition between these two states is governed by a logistic function H_t^{AG} that depends on the variable z_t, which is defined as the centred moving average (MA) of order 7 of the growth rate of real GDP.

Despite an apparently similar framework, the results generated by the two different estimation approaches (the model described by equation 2.7 and those described by equations 2.1 and 2.5) are very different: Auerbach and Gorod-

[27] Ramey and Zubairy (2014) propose a different estimation method using local projections (see Section 4.5) and historical data from 1889 to 2011. They find no significant differences in responses during periods of B and R. However, when their methodology and data are used for the post-War period used in this chapter, the results are very similar to those presented in Section 2.2 (with output contracting after positive government spending shocks during times of R).

nichenko (2012) find that a government spending shock that occurs during a time of R has a positive and larger effect than the same shock occurring during a time of B.

Why do similar estimation methods yield such contrasting results? The answer to this question rests on the information used to determine the current state of the economy. Equation 2.7 uses a continuous variable determined by a centred MA of the growth rate of real GDP, while equation 2.1 includes a binary variable that follows the NBER definition of recession.[28] Constructing H_{t-1}^{AG} in equation 2.7 in such a way has potentially important implications. By using a centred MA of order j (a two-sided MA filter), at any given period of time, we are making use of future developments in GDP to inform about the current state of the economy. For example, in period t, whether the economy is in recession or expansion will be determined by information up to period $t + (j - 1)/2$. In the event of an incoming change in the business cycle (e.g. from an expansion to a recession), we could potentially be mislabelling the current state of the economy.

In order to determine whether the nature of the two-sided MA filter can explain the differences between the two sets of results, I replicate the benchmark analysis in Auerbach and Gorodnichenko (2012) for different sizes of the centred MA of the growth rate of real GDP. Figure 2.10 shows the responses of GDP and government spending to a positive shock to the latter during times of B and R when varying the size of the MA from 5 up to 19. The results suggest that the impact of the shock on GDP does depend on the size of the MA filter: using a high-order MA (i.e. using more information that has not yet occurred) reduces the effect of the shock during times of B (with the effect even becoming negative in the medium run) and augments it during times of R.

[28]When I redefine H_{t-1}^{AG} in equation 2.7 to be a dummy variable, the results are qualitatively similar.

When the size of the benchmark specification in Auerbach and Gorodnichenko (2012) (a MA of order 7) is reduced to a MA of order 5, the results become qualitatively the same as those described earlier in this chapter: a government spending shock has a positive effect on GDP during times of B and a negative effect during times of R.

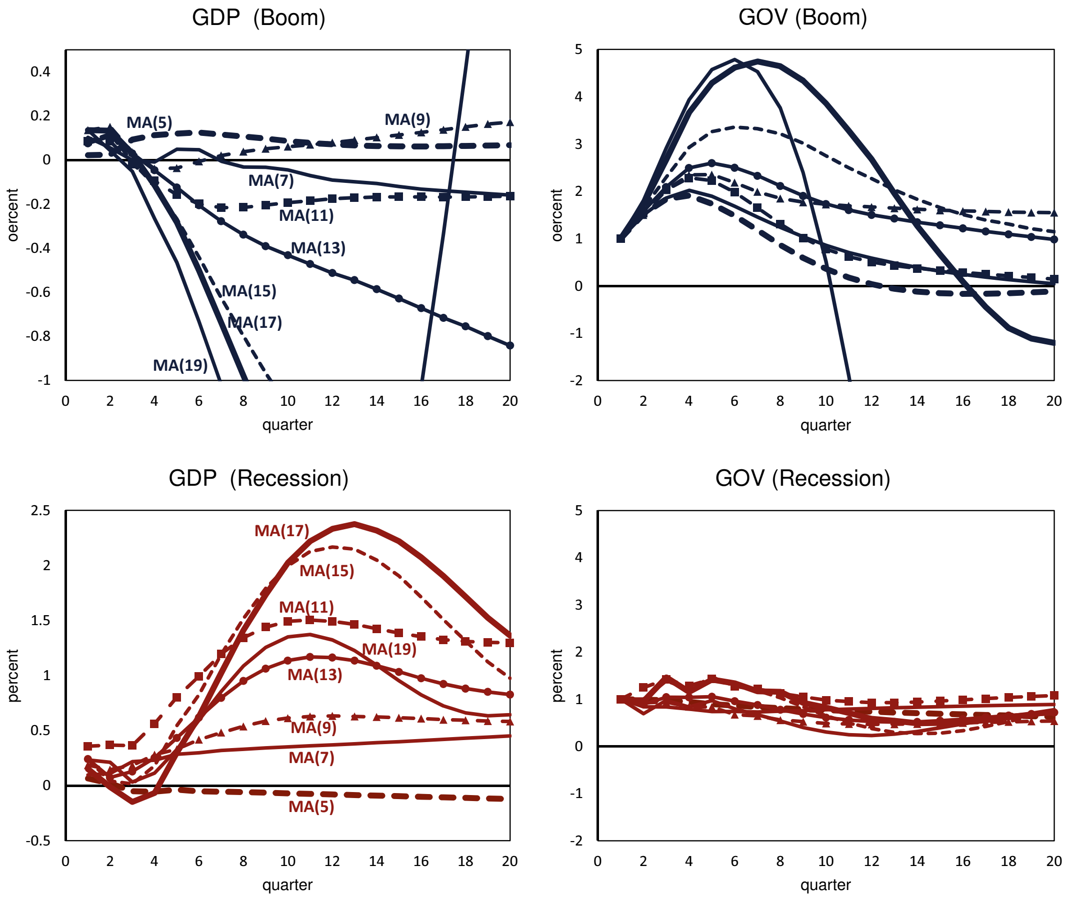
Next, I analyse how the results in Auerbach and Gorodnichenko (2012) would be affected if the centred MA were substituted by a one-sided MA filter (i.e. keeping the length of the MA filter constant, but altering its symmetry). Figure 2.11 shows the responses to a government spending shock when I use (i) the benchmark specification (centred MA of order 7), (ii) a one-sided MA filter of order 7 that only uses past information and (iii) a one-sided MA filter that exclusively uses future information.[29] The results confirm that, when not using information about the future (i.e. when the MA is only backward-looking), the response of output becomes more similar to those obtained from equations 2.1 and 2.5: a government spending shock has positive effects during a period of B, but negative ones during a period of R (while the opposite is true when a forward-looking MA filter is used).

I conclude that the differences between the results presented in this chapter and those in Auerbach and Gorodnichenko (2012) respond to the information used to explain changes in the state of the economy.[30]

[29]Cases in between these two extremes (e.g a MA(7) filter that uses information from the last two quarters and the next four quarters) support the same conclusions.

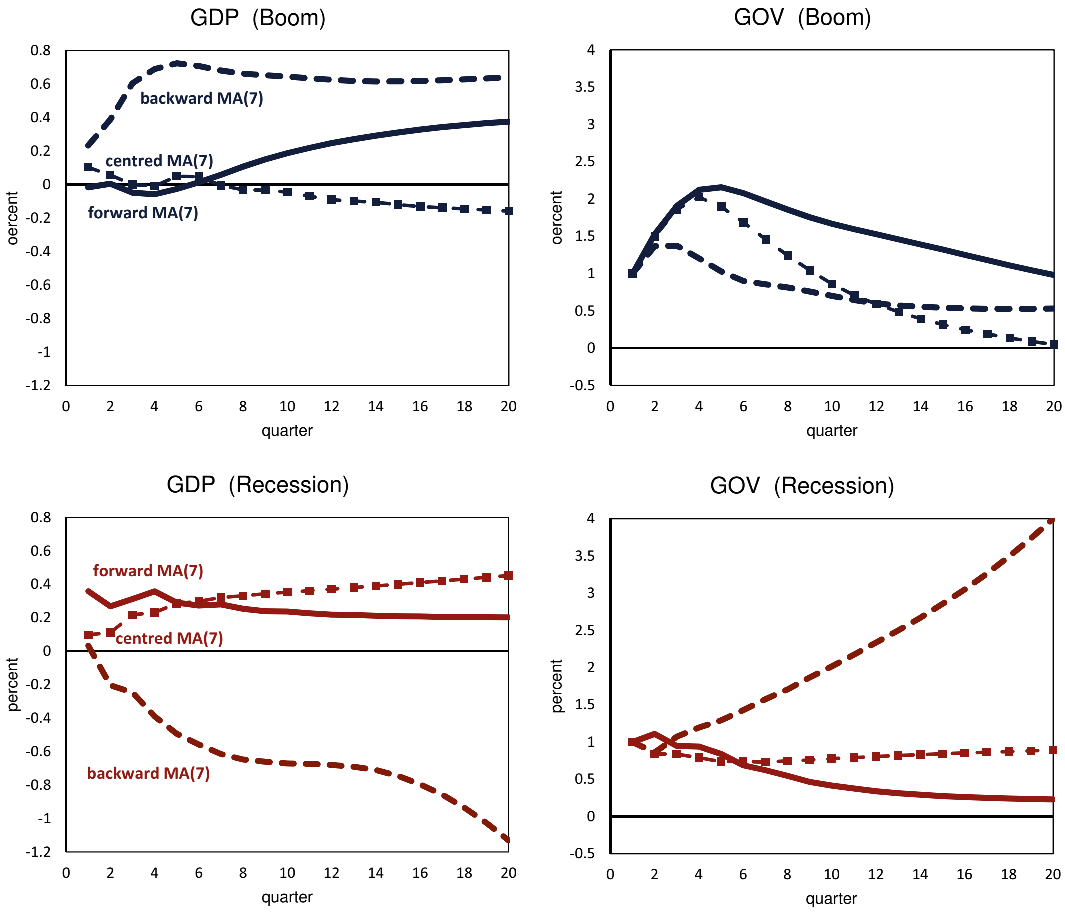
[30]Bognanni (2012) uses a Markov-switching VAR where the probability of recession is estimated period by period, and finds that the effect of a government spending shock on activity is smaller during periods of R than B.

Figure 2.10: Responses during times of R and B using the Auerbach and Gorodnichenko (2012) framework and a two-sided MA filter



The top panel (in blue) shows responses to a government spending shock during times of boom. The bottom panel (in red) shows responses during times of recession. Note that the graphs in the left column have a different scale to facilitate their readability. The responses are computed using the strategy described in Auerbach and Gorodnichenko (2012) but with variations in the size of the centred moving average of the growth rate of real GDP used to provide information about changes in the regime.

Figure 2.11: Responses during times of R and B using the Auerbach and Gorodnichenko (2012) framework and a one-sided MA filter



The top panel (in blue) shows responses to a government spending shock during times of boom. The bottom panel (in red) shows responses during times of recession. The responses are computed using the strategy described in Auerbach and Gorodnichenko (2012) but with variations made to the centring of the moving average (MA) of the growth rate of real GDP used to provide information about changes in the regime. Forward MA(7) is a one-sided MA filter of order 7 using future information only; backward MA(7) is a one-sided MA filter of order 7 using exclusively past information.

2.3 Robustness

2.3.1 Alternative Timing of Uncertainty Events

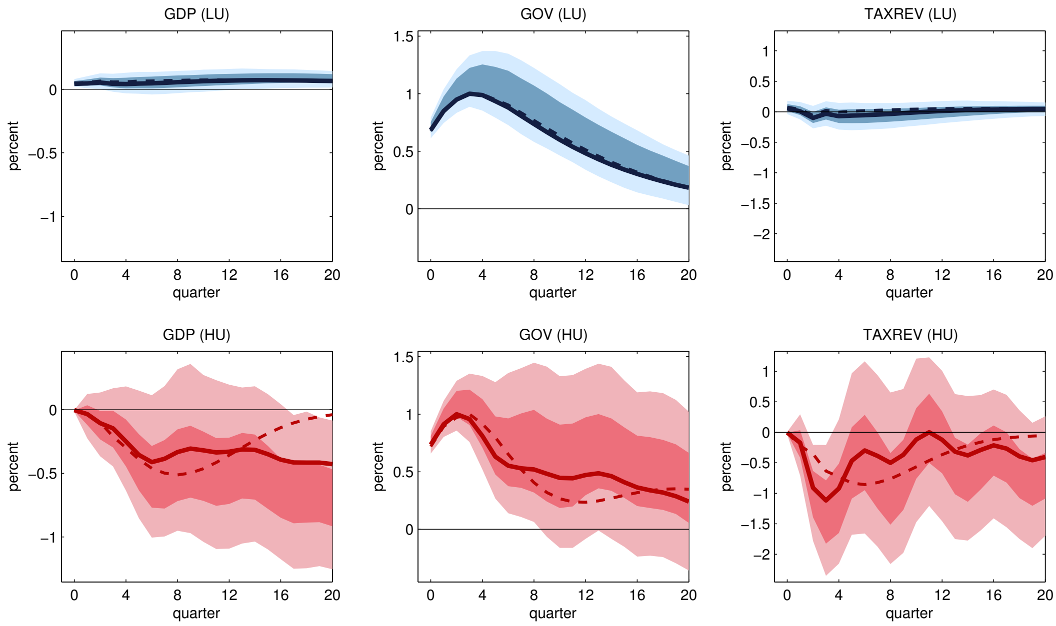
In this subsection I analyse the sensitivity of the results to changes in the definition of the episodes of HU. In Section 2.2, these were defined as quarters containing any month with unusually high stock market volatility (1.65 standard deviations above a Hodrick-Prescott trend). The advantage of this approach is that it produces a larger number of periods of HU (32 quarters). I now strictly apply the definition of episodes of HU in Bloom (2009) and consider only those quarters that contain months of peak volatility.[31]

To assess whether this change of definition affects the results, I estimate equations 2.1 and 2.5 again using the alternative definition of HU. Figure 2.12 displays the results of a government spending shock identified using the SVAR approach (equation 2.1). The response of the variables during times of LU is almost unchanged between the alternative (solid line) and benchmark (dashed line) definition of episodes of uncertainty. During times of HU, the response of output is fairly similar for both definitions during the first two years after the shock. The most noticeable difference is that the alternative definition produces more persistent effects of the shock.

Next, I consider the case of the narrative identification of government spending shocks (equation 2.5). Figure 2.13 plots the results for this case. The responses of the variables during LU is are almost identical for both definitions of uncertainty. When considering times of HU, the response of output is again very similar in each case during the first two years. The magnitude

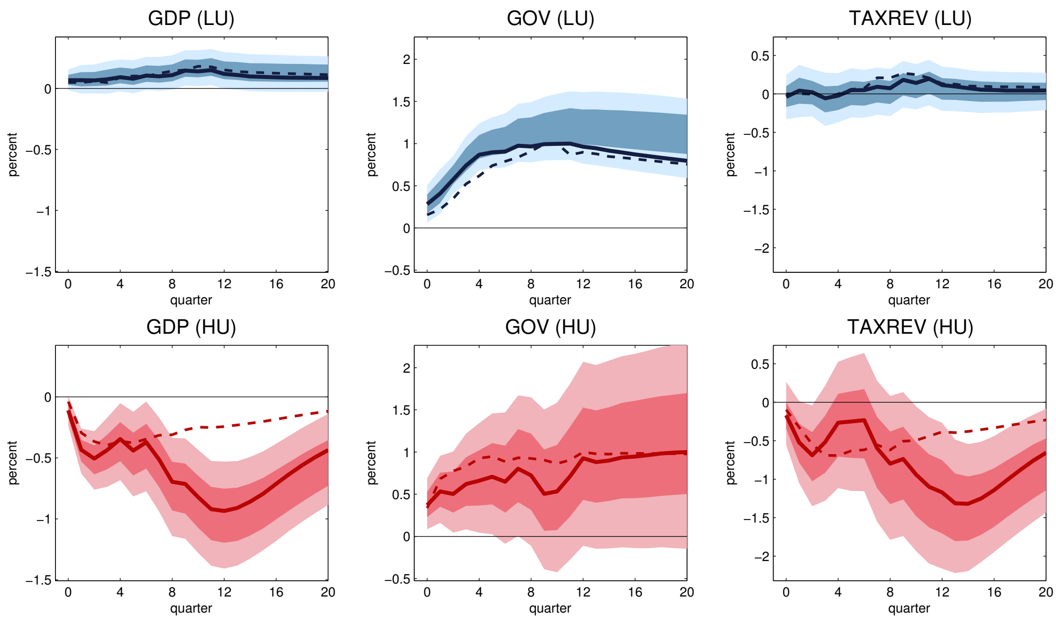
[31]For example, from 2002 Q3 to 2003 Q1 stock market volatility was 1.65 standard deviations above the trend, due to the second Gulf War. In the benchmark definition of episodes of HU this resulted in three quarters of HU. In the alternative definition proposed now, I only consider the quarter with the highest volatility (in this case, 2003 Q1).

Figure 2.12: Responses under alternative definition of HU events (SVAR identification)



The top panel (in blue) shows responses to a government spending shock (identified from exclusion restrictions) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. The solid line plots the point estimates for the alternative definition of HU events. The dashed lined plots the point estimates for the benchmark definition used in Section 2.2. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

Figure 2.13: Responses under alternative definition of HU events (Narrative identification)



The top panel (in blue) shows responses to a government spending shock (identified from narrative accounts of defence spending) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. The solid line plots the point estimates for the alternative definition of HU events. The dashed lined plots the point estimates for the benchmark definition used in Section 2.2. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

of the decline in output is larger for the alternative definition (dashed line) between the second and fifth years of the horizon.

We can conclude that the finding of different signs of the responses of output during times of HU and LU is robust when we consider uncertainty episodes as defined by quarters of peak stock market volatility.

2.3.2 Local Projections

Equations 2.1 and 2.5 both imply restrictions on the responses of the variables to structural shocks. Responses are linear when conditioning on a given state and are therefore history-independent. This is equivalent to assuming that fiscal policy, through government spending, cannot change the regime from HU to LU (or from R to B) or vice versa. While uncertainty is defined here as exogenous events (most of the episodes of HU are not economics-related), these events are mostly short-lived. It is also plausible to believe that government spending can influence the economic situation. Although these shortcomings are less likely to appear in the short run, I follow Auerbach and Gorodnichenko (2013) and Ramey and Zubairy (2014) in using a methodology that takes these issues into account. I use the local projection methodology proposed in Jordà (2005), which relaxes the assumption that the state of the nonlinear model remains fixed throughout the entire horizon of the impulse response analysis.

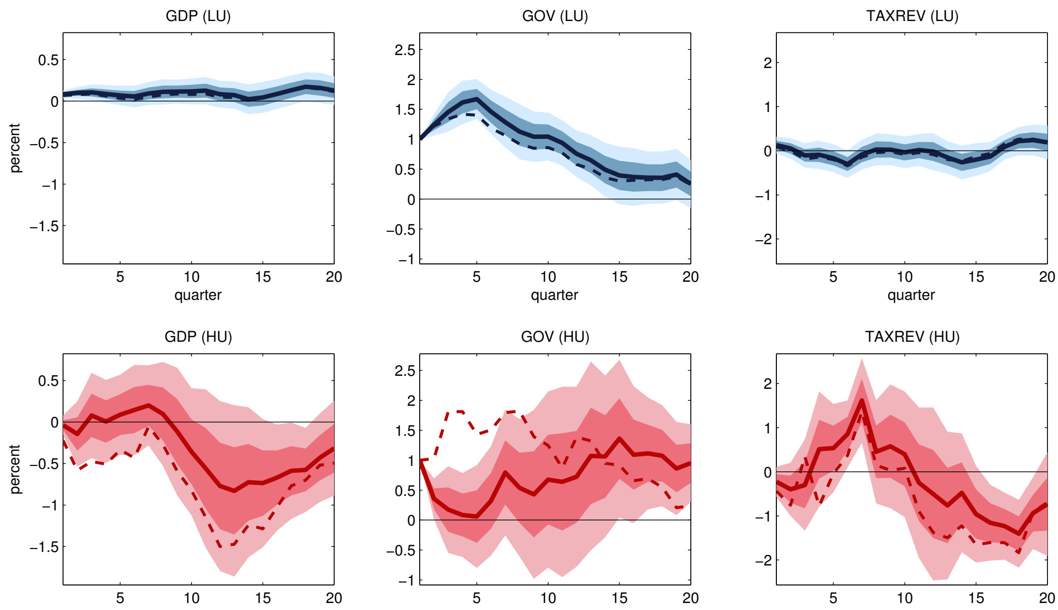
The local projection methodology estimates a series of single equations over the horizon h:

$$\begin{aligned} x_{t+h} = H_{t-1}\left[\alpha_{L,h} + \beta_{L,h}\boldsymbol{x}_{t-1} + \delta_{L,h}\varepsilon_t^G\right] + \\ (1 - H_{t-1})\left[\alpha_{H,h} + \beta_{H,h}\boldsymbol{x}_{t-1} + \delta_{H,h}\varepsilon_t^G\right] + \quad e_{t+h} \end{aligned} \tag{2.11}$$

where $\boldsymbol{x}_t = [g_t, y_t, tr_t]'$ is the vector of variables defined in the previous section and x_t is one of the variables of interest in $\boldsymbol{x}_t$. Note that equation 2.11 allows for the coefficients to change for each horizon h. The coefficient $\delta_{L,h}$ measures the response of the variable x_t to a government spending shock ε_t^G during state L (which represents times of LU or B) and, conversely, $\delta_{H,h}$ captures the response during state H (times of HU or R). The responses of the variable of interest to government spending shocks during state L (or H) are given by a series of $\delta_{L,h}$ (or $\delta_{H,h}$) obtained from each regression h.

I apply the above method using both of the identification schemes mentioned earlier. Thus, ε_t^G represents either the government spending variable (to achieve the Blanchard-Perotti SVAR identification) or Ramey's news about defence spending (narrative identification).

Figure 2.14: Responses during times of LU and HU computed using local projections (SVAR identification)

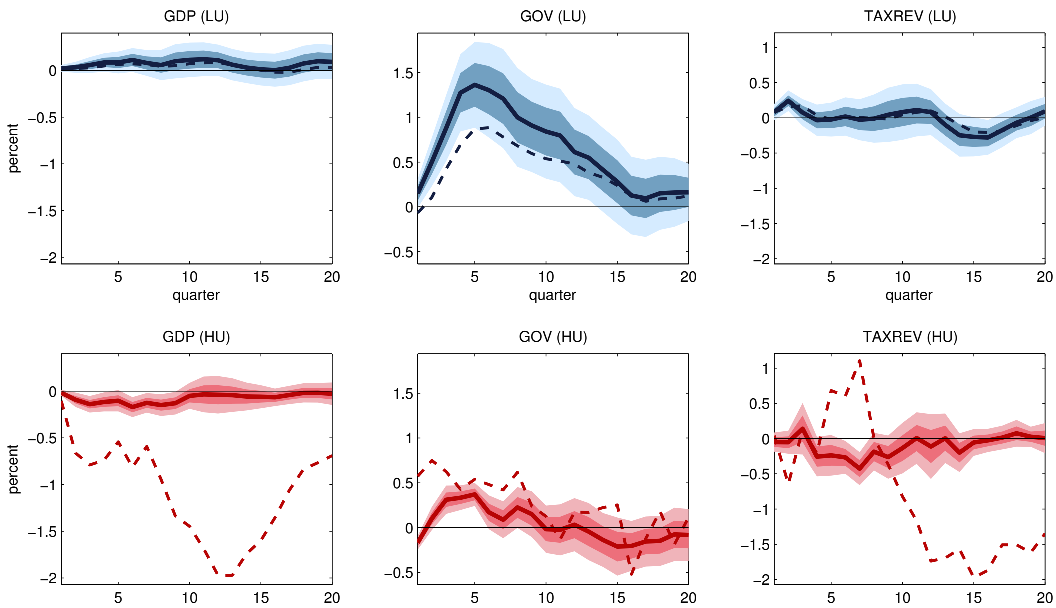


The top panel (in blue) shows responses to a government spending shock (identified using exclusion restrictions) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. Responses are computed using the local projection method as described in Jordà (2005). The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

Figure 2.14 shows the responses to a government spending shock identified as in Blanchard and Perotti (2002) when equation 2.11 is allowed to vary between states of HU or LU. The response of output is positive and significant during times of LU. During times of HU, the point estimate (solid line) is not significant during the first two years and then becomes negative. This figure also displays the point estimate when the episodes of HU are identified using the alternative measure of uncertainty explained in the previous subsection (dashed line). This estimate is negative and significant (confidence intervals for this estimation are not shown) throughout the entire impulse response horizon.

I now replicate the same analysis but using the narrative identification ap-

Figure 2.15: Responses during times of LU and HU, computed using local projections (narrative identification)



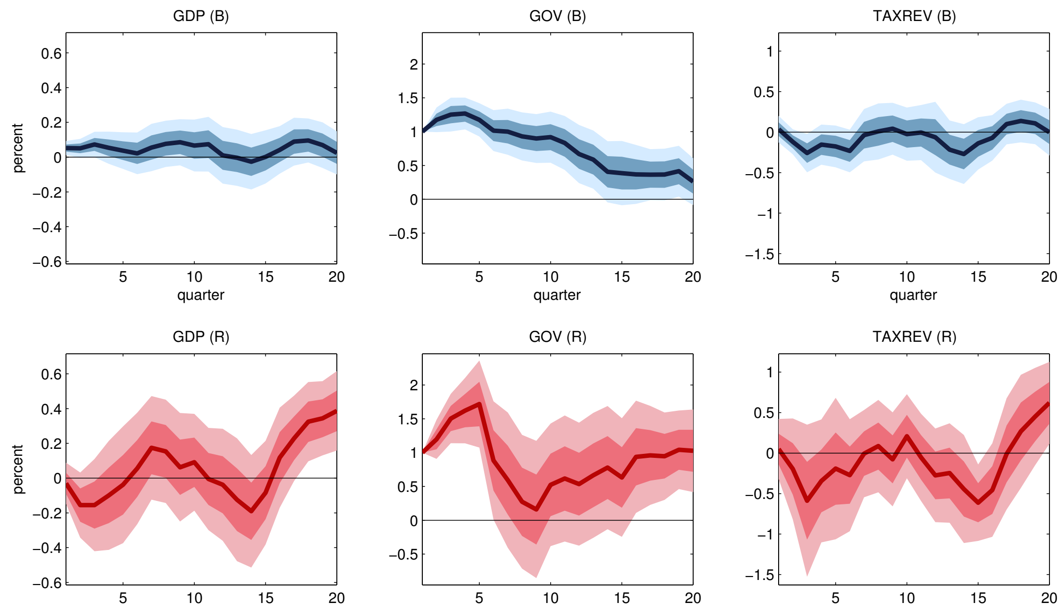
The top panel (in blue) shows responses to a government spending shock (identified from narrative accounts of defence spending) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. The responses are computed using the local projection method as described in Jordà (2005). The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

proach for the government spending shock. Figure 2.15 displays the responses. As in the previous case, the response of output is positive and mostly significant during times of LU. The sign of this response becomes negative and significant when the shock happens during a time of HU. The same result obtains when we consider the alternative definition of HU (dashed line) although the magnitude of the decline in output is significantly larger.[32]

Finally, I consider the business cycle conditions (R or B) to be the drivers of the nonlinearities in equation 2.11. Figure 2.16 plots the responses to government spending shocks identified using the SVAR framework. In times of B,

[32]Note that these responses, unlike those shown in Section 2.2, are not scaled to the government spending shock, since we are estimating single equations.

Figure 2.16: Responses during times of B and R, computed using local projections (SVAR identification)



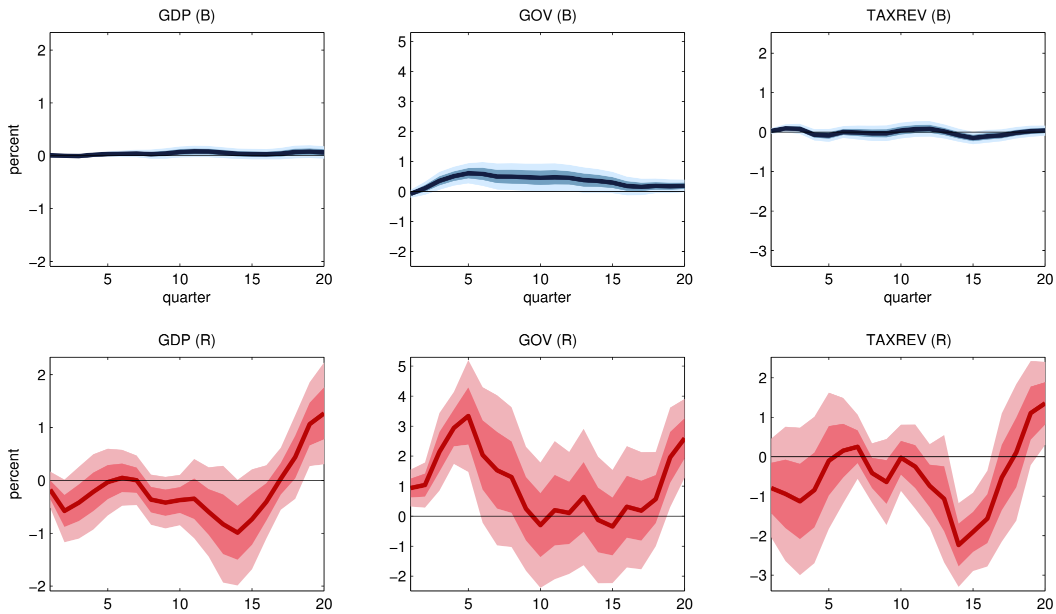
The top panel (in blue) shows responses to a government spending shock (identified using exclusion restrictions) during times of boom. The bottom panel (in red) shows responses during times of recession. The responses are computed using the local projection method as described in Jordà (2005). The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

the response of output is positive and mostly significant. During times of R, the response is negative during the first year but only significant up to 68%, and then fluctuates for the rest of the horizon considered (increasing after the fourth year).

Figure 2.17 displays the responses when the government spending shock is identified following the narrative approach. The response of output is positive but small during times of B. During times of R, output declines during the first year and remains negative or close to zero up to the fourth year (after which it increases), although the levels of significance are low.

To summarise, the local projection framework shows that the nonlinear responses due to different levels of uncertainty are not caused by the restriction

Figure 2.17: Responses during times of B and R, computed using local projections (narrative identification)



The top panel (in blue) shows responses to a government spending shock (identified from narrative accounts of defence spending) during times of boom. The bottom panel (in red) shows responses during times of recession. The responses are computed using the local projection method as described in Jordà (2005). The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

imposed by the VAR (i.e. the state remaining fixed for the whole impulse response horizon). The results are, however, less clear when we consider non-linearities due to the state of the business cycle.

2.3.3 Alternative Identification Strategy

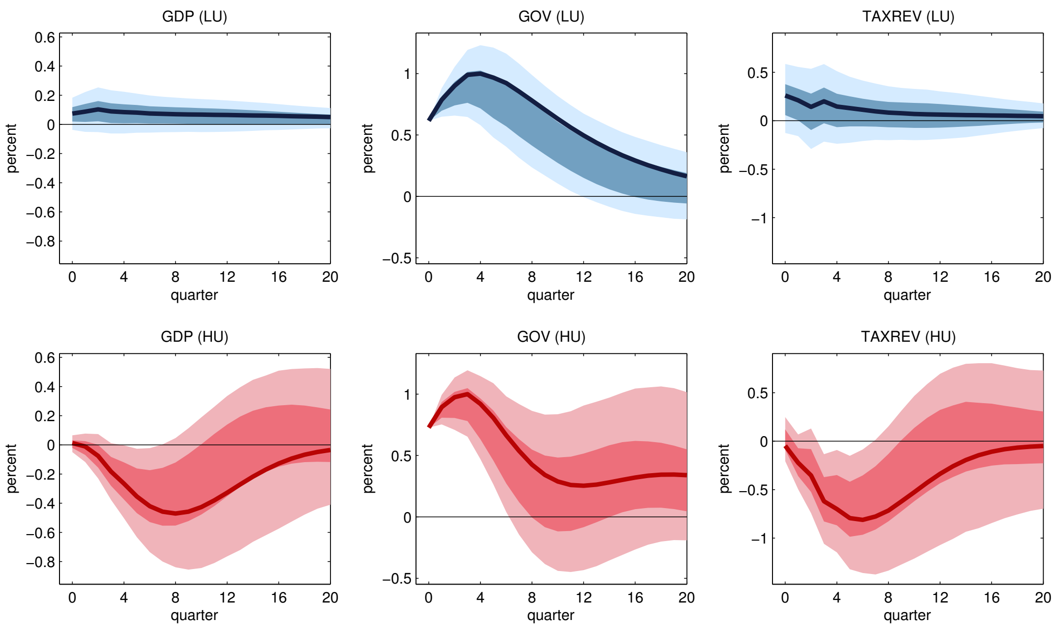
In this subsection I use a different strategy to identify the government spending shocks. In Section 2.2 I used the Ramey (2011a) narrative shock of news about defence spending as the true structural shock to government spending. One might suppose that this series could be contaminated by measurement errors. I now follow Mertens and Ravn (2013) and consider the possibility that the narrative shocks are not the true structural shocks but a measure correlated with them. I assume as well that this measure, the proxy, is orthogonal to other structural shocks. These assumptions give us extra identification restrictions (exploiting the correlation between the proxy and the reduced form residuals of the SVAR) that can be used in conjunction with the covariance restrictions from the VAR in equation 2.1 to identify the structural shocks to government spending.[33]

Figure 2.18 plots the responses when using this alternative identification, for the case of HU versus LU. The response of output follows a similar pattern to that in the benchmark results: it increases as a result of a government spending shock during a time of LU but becomes negative when the shock takes place during a time of HU, with a magnitude very similar to the results presented in Section 2.2.

Next I consider the case of responses to government spending shocks during

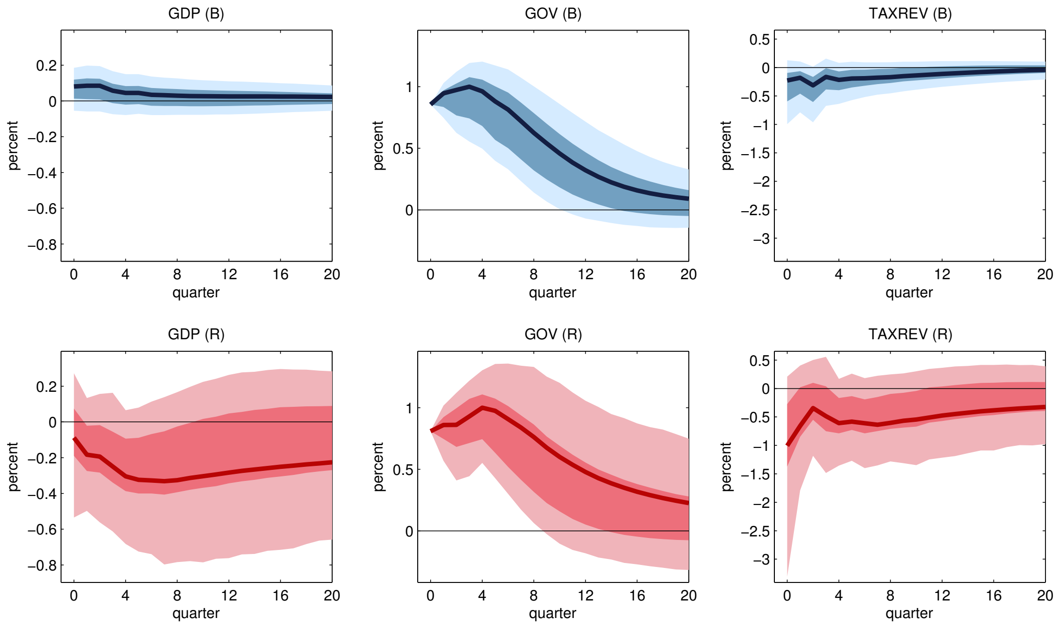
[33]Note that we no longer need the Blanchard and Perotti (2002) exclusion restrictions on the contemporaneous impact of a government spending shock in order to achieve identification.

Figure 2.18: Responses during times of LU and HU (proxy identification)



The top panel (in blue) shows responses to a government spending shock during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. Government spending shocks are identified using the Ramey (2011a) news about defence spending as a proxy for the true structural shocks. The 68% and 95% confidence bands are computed using the Gonçalves and Kilian (2004) wild bootstrap.

Figure 2.19: Proxy identification, R versus B



The top panel (in blue) shows responses to a government spending shock during times of B. The bottom panel (in red) shows responses during times of R. Government spending shocks are identified using the Ramey (2011a) news about defence spending as a proxy for the true structural shocks. The 68% and 95% confidence bands are computed using the Gonçalves and Kilian (2004) wild bootstrap.

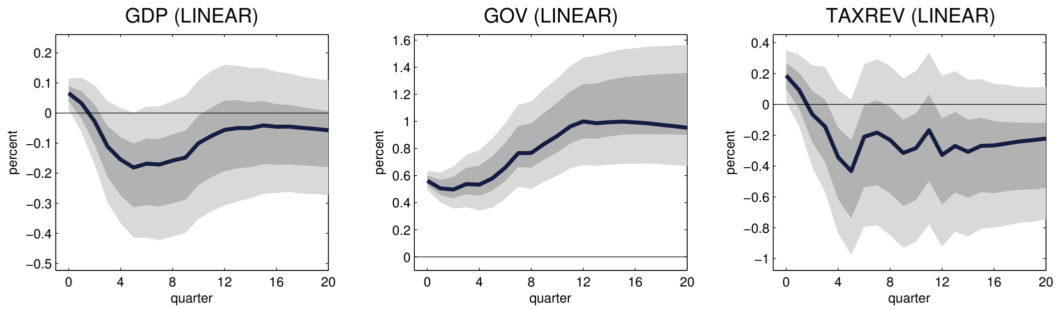
times of R or B.[34] Figure 2.19 shows that the response of output is positive during times of B. Output declines during times of R, with an implied elasticity of the output peak to the government spending peak of -0.3% after the first year. The magnitude of this response falls between the results obtained in Section 2.2 for the SVAR and narrative identification approaches.

2.3.4 Alternative Narrative Measure

In Section 2.2 I used the Ramey (2011a) news about defence spending as the government spending shocks. Ramey (2011a) produces a second narrative

[34] I find that the results during times of B are very imprecisely estimated when we include the recession of 1949. In this particular case I consider 1950 Q1 as the starting date of the sample. The results for the case of Rare not dependent on this consideration.

Figure 2.20: Responses in the linear case (alternative narrative identification)



Responses in the linear model after a government spending shock identified using Ramey (2011a) defence spending forecast errors. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

measure: news shocks based on the one-quarter-ahead forecast error of defence spending by professional forecasters.[35] Ramey (2011a) reports that shocks to this variable are associated with decreasing output.[36] I find similar results when I estimate the linear version of equation 2.5 with these shocks (as shown in Figure 2.20).[37]

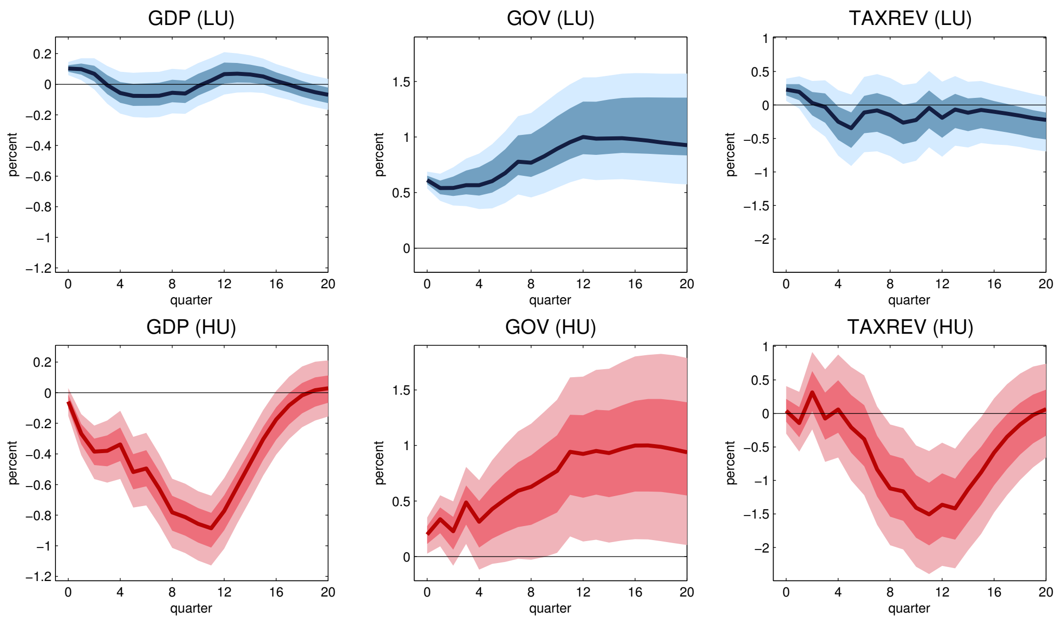
I want to investigate whether the response of output is still qualitatively different across states when we consider this alternative identification of government spending shocks. I estimate equation 2.5, allowing for nonlinearities due to periods of HU or LU but using the new narrative measure. Figure 2.21 shows the responses after a government spending shock in this case. During times of LU the response of output is positive on impact, before becoming negative and then insignificant (the point estimation becomes positive again after the third year, in contrast with the linear case displayed in Figure 2.20

[35]Ramey (2011a) suggests that the explanatory power of the defence news variables may be lower when the sample starts after WWII and the Korean War (I include only the latter), the two major events of increases in government spending.

[36]Ramey says: "*these shocks lead to rather contractionary effects, similar to those I found for the 1955 to 2008 period with my defence news shocks*", a statement that I have illustrated in Figure 2.5.

[37]The sample starts in 1969 Q1, restricted by the availability of the data.

Figure 2.21: Responses during periods of HU and LU (alternative narrative identification)

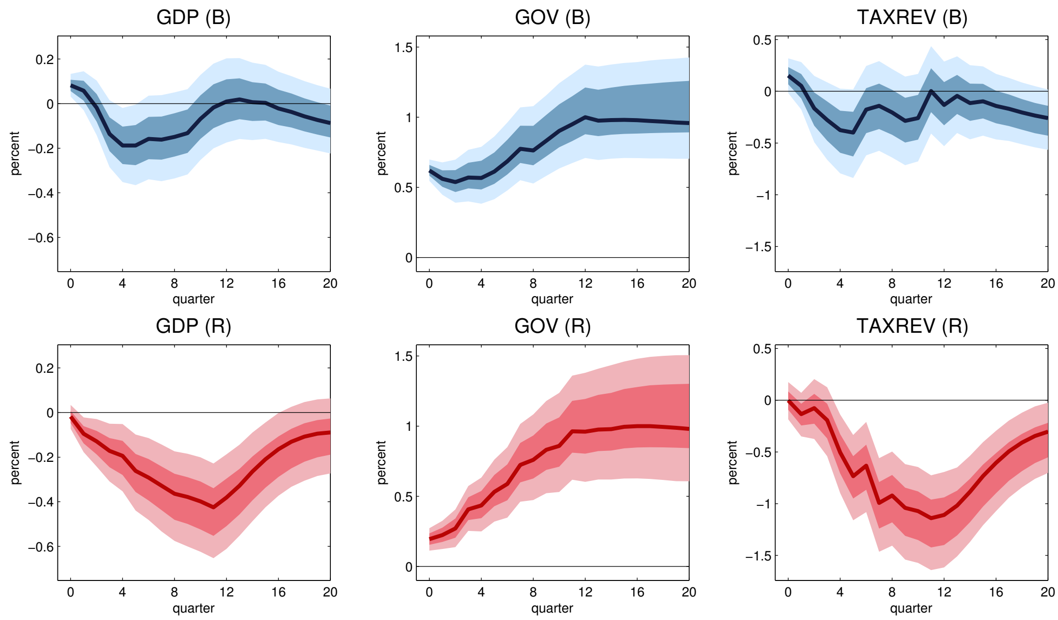


The top panel (in blue) shows responses to a government spending shock during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. Government spending shocks are identified using Ramey (2011a) defence spending forecast errors. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

in which it remained negative during the entire period). When considering the response of output during times of HU, we again observe a large and significant decline following the shock, as in the results presented in Section 2.2.

I now consider the case of the impact of a government spending shock during times of R and B. Figure 2.22 shows the responses for this case. We again observe an initially positive response of output during times of B, becoming negative (and then insignificant) after the third quarter. The response of output during times of R is negative and significant, as observed in the benchmark results.

Figure 2.22: Responses during periods of R and B (alternative narrative identification)



The top panel (in blue) shows responses to a government spending shock during times of boom. The bottom panel (in red) shows responses during times of recession. Government spending shocks are identified using Ramey (2011a) defence spending forecast errors. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

2.4 Conclusion

The effect of government spending is likely to depend on features of the economy that evolve over time. In this chapter, I study whether the effects of changes in government spending remain the same across states of the economy. In particular, I empirically characterise how changes in government spending may differ across states of high (HU) and low (LU) uncertainty and across recessions (R) and booms (B).

The results suggest that the impact of government spending shocks on output is positive during times of LU or B and negative during times of HU or R. I find that households' confidence is a key variable for interpreting these results, as agents become more pessimistic when a positive government spending shock confirms their views on the state of the economy.

Other studies in the literature (such as Auerbach and Gorodnichenko (2012)) produce contrasting results. I explore these differences by highlighting the importance of the information used to determine the state of the business cycle.

The results documented here provoke new research questions. For example, we have seen that output contracts after a positive government spending shock if that shock happens during a time of HU or R. It would be interesting to identify whether it is HU, R or a combination of both that is causing this effect. This would require a comparison between a shock that happens in a time of HU and B and a shock that happens during a time of LU and R. However, the data are not informative enough for this, since there are just a few events with these characteristics, insufficient for us to obtain robust results. More empirical evidence is required to help us shed light on this question.

It is also necessary to understand the mechanism causing these differing impacts of government spending on the economy. Here I have highlighted the importance of households' confidence in explaining the results. A detailed theoretical framework that can explain such nonlinear effects would be crucial for evaluating the consequences of public policies.

I have focused my attention on uncertainty that has an arguably exogenous origin (e.g. war, terror). However, uncertainty can be generated by endogenous causes, for example by policy itself (see Baker et al. (2013), Fernández-Villaverde et al. (2011) or Bi et al. (2013)).[38] Whether and how this source of fiscal uncertainty can affect real activity are questions left for future research.

[38]Uncertainty derived from fiscal policy has received attention from the media recently. See, for example, The Economist (16/11/2013):"*Governments, however, are still breeding fears about the future. The most glaring form of uncertainty in the rich world is fiscal. [...] This is self-imposed uncertainty. If the fiscal path were a little clearer, the reduction in uncertainty should spur investment and output, which in turn should improve the fiscal picture.*"

Chapter 3

The Impact of Taxes on Income Mobility

3.1 Introduction

The last four decades have witnessed a sustained increase in income and wealth inequality in the US, particularly at the top end of the distribution.[1] This phenomenon has received substantial attention in academic research,[2] policy debates as mentioned in President Obama's Economic Report (see Council of Economic Advisers (2015)), and popular opinion (e.g. protest movements such as Occupy Wall Street). As noted in Arrow and Intriligator (2015), inequality is a highly relevant normative issue, since society perceives an unequal distribution of income as an unjust outcome of market economies. However, there are other features of the income distribution beyond inequality that have welfare implications for the society and are relevant from a policy point of view. Over-

[1]See Piketty and Saez (2003) for long-run trends in income inequality and Saez and Zucman (2014) or Quadrini and Ríos-Rull (2015) for the case of wealth.

[2]Piketty (2014) provides extensive evidence of income and wealth inequality around the world while Stiglitz (2012) highlights its consequences: "the impact of inequality on societies is now increasingly well understood -higher crime, health problems, and mental illness, lower educational achievements, social cohesion and life expectancy" (inside cover).

looking some of these aspects may yield an incomplete or inaccurate picture of the effects of policies that address economic disparities.

This chapter looks at the impact of fiscal policy on another aspect of the income distribution different to inequality. Particularly, I investigate the relationship between taxes and income mobility. While inequality reflects changes in the variance (and higher moments) of the income distribution, income mobility is the result of variations in the covariance of income between two periods of time.[3] For any given level of income inequality, mobility reduces the association between the positions of origin and destination in the income distribution, increasing equality of opportunity. Therefore, to the extent that income mobility is a desirable feature of an economy, it is then relevant to consider how fiscal policy may affect it.[4]

I analyse the impact of taxes on the probability of moving in the income distribution in the US using data from the Panel Study of Income Dynamics (PSID). I measure income mobility as changes in the relative position of households in the income distribution (i.e. changes in deciles or quintiles) across time. Income is defined as the Adjusted Gross Income (AGI) of the household. I assess the degree of mobility across three specifications for the income distribution that consider pre-tax income, post-tax income and post-tax and post-transfer income respectively. Then, I construct the federal individual tax liabilities faced by each household in the sample using the NBER TAXSIM simulator. With these data in hand, I estimate a linear probability model to understand how changes in the marginal tax rate affect the likelihood that households are mobile in the income distribution during two adjacent years. My identification approach accounts for endogeneity in the marginal rates by

[3]See Gottschalk (1997).

[4]Kopczuk et al. (2010) argue for the need to study income inequality and mobility jointly. Income mobility is a determinant of inequality in the long run: when there is no mobility in the income distribution, short-run inequality perpetuates.

isolating the variation in taxes that is only due to legislative changes. I exploit this source of exogenous variation as an instrument in the regressions.

The results obtained suggest that higher marginal tax rates reduce income mobility. Particularly, I find that an increase of one percentage point in the marginal rate is associated with declines of about 0.5-1.3% in the probability of changing deciles of income. A decrease of 7 percentage points in the marginal tax rate (slightly smaller than a standard deviation of non-zero changes in the rates) can account for about a tenth of the average income mobility in a year. The effect of taxes on mobility arises in specifications that consider income distributions both before and after taxes and transfers, suggesting that the impact of taxation on mobility goes beyond redistribution effects. The economic mechanism that induces this impact seems to be related to the labour market incentives created by changes in the tax schedule. Additional results suggest that the effect of taxation on income mobility differs according to the level of human capital (measured as the education of the head of the household) and that it is particularly significant when considering mobility at the bottom of the distribution.

The evidence that taxes have a negative impact on income mobility has important implications for the design of policies that aim to address economic disparities. While some studies have pointed out to the importance of progressive taxation in addressing inequality,[5] the results from this paper suggest that such changes may have a detrimental impact on income mobility. Therefore, the design of optimal fiscal policy should consider the overall impact on welfare of the trade-off that might arise when jointly addressing income inequality and mobility.

This chapter relates to different strands of literature. First, it is connected

[5] See Piketty and Saez (2007) and Diamond and Saez (2011).

to the literature that investigates the effects of tax changes on taxable income (elasticity of taxable income, or ETI), as reviewed in Saez et al. (2012). This research finds that taxable income only reacts moderately to changes in the marginal tax rate.[6] Mertens (2013) suggests that accounting for empirical difficulties in the estimation of the ETI at the aggregate level (such as policy endogeneity or timing) results in larger elasticities for different income groups beyond the top earners. This chapter relates to some methodological aspects of this literature,[7] but I focus on the effects of taxes on measures of household's mobility across the income distribution as opposed to the individual's response of reported taxable income (for this purpose I employ a different type of data, the PSID, that allows me to control for relevant demographic factors).

This paper relates to an extensive literature on income mobility surveyed in Fields and Ok (1999) and Jäntti and Jenkins (2015).[8] Early works on the measurement of income mobility include Shorrocks (1978a) and Shorrocks (1978b), which lay down many of the tools currently used to measure mobility. A number of papers have investigated the degree and evolution of mobility in terms of income (broadly defined) and earnings. Hungerford (1993) uses family income data from the PSID to analyse trends in mobility, focusing on changes in the position in the income distribution between 7 year-periods in both annual

[6] Saez et al. (2012) suggest a range of estimates from 0.12 to 0.40 for the ETI. The authors argue that responses for the top-earners can be substantially higher. For example, Slemrod (1996) finds that the Tax Revenue Act of 1986 explains to a large extent the increase in reported income of the top earners.

[7] I use variation in legislated taxes to address endogeneity following Gruber and Saez (2002).

[8] During this chapter, I will refer to income mobility as intragenerational mobility. Jäntti and Jenkins (2015) also survey the literature on intergenerational or social mobility (the degree of association between parents and children income). There has been a recent increase in the research aiming to understand the degree of intergenerational mobility and its factors. For example, Chetty et al. (2014a) analyses the geographical differences of intergenerational mobility in the US and Chetty et al. (2014b) explores its evolution over time, which has remained fairly constant despite rising inequality. The determinants of social mobility are explored in Chetty and Hendren (2015), who investigate how neighbourhoods affect intergenerational mobility through childhood exposure effects.

and permanent income. The author compares mobility between the 1970s and 1980s to find considerable movement within the income distribution (although he finds less evidence of sizeable upward or downward movements). Also using PSID data, Gottschalk (1997) looks at earnings mobility in one-year and seventeen-year periods. The author concludes that the degree of mobility is high enough to support the view that people are not stuck at the bottom or the at top of the distribution. Kopczuk et al. (2010) employs individual data from the Social Security Administration to investigate the evolution of both short-term mobility (measured by changes in rank correlation in year-to-year earnings and in mobility indices defined over periods of 3-5 years) and long-term mobility (i.e. across the working life). More recently, Bradbury (2011) looks at various indices of income mobility using the PSID between 1969-2006 and time intervals spanning 11 years.

Although the concepts of mobility and samples used differ, these papers find a similar evolution of mobility in the US: a relatively flat profile during the 1970s and a somewhat decreasing trend after that. I measure mobility in comparable ways to this literature, however, since my main goal is to identify the effects of tax changes, I instead consider mobility across two adjacent years.[9]

The literature on the effect of taxes on income mobility is more limited. Lerman and Yitzhaki (1995) analyse the effects of the 1991 tax reform on the income distribution recognising two potential channels: higher taxes can reduce the income gaps between people and, in some cases, change their relative position by means of redistribution. The authors decompose the evolution of the Gini coefficient due to income changes holding the relative position constant

[9]Gottschalk (1997) notes that accounting for longer periods is not necessarily more appropriate than one-year periods to analyse mobility and inequality, given the fact that low-income households are more likely to face borrowing constraints over longer horizons.

and due to changes in the relative position holding income constant, and find that this second effect is important in understanding the redistribution effects of the the 1991 tax reform. Larrimore et al. (2015) analyse the determinants of income mobility between two-year periods using a panel of tax returns between 1999 and 2011. They compute the difference between income before and after federal taxes as a measure of the stabilising power of taxes.

In contrast to both Lerman and Yitzhaki (1995) and Larrimore et al. (2015), I analyse the effects of taxes on mobility that can also be due to changes on the pre-tax income (i.e. because of a change in the labour supply), not only due to the redistribution effect of the tax system. More substantially, this chapter uses a different methodology to asses the impact of taxes on income mobility by exploiting exogenous variation in the marginal tax rates, over a relatively long panel of data that includes several tax reforms.

This chapter also relates to the literature that investigates the aggregate impact of taxes such as Romer and Romer (2010) and Barro and Redlick (2011). Both studies find substantial effects of changes in taxes on economic activity. Romer and Romer (2010) estimate the impact of a tax increase of 1 percent of GDP to amount to a reduction of output by 3% over the course of three years. Barro and Redlick (2011) find that the effect of taxes on GDP act mainly through substitution effects, with increases in the average marginal tax rates significantly reducing GDP.[10] Mertens and Ravn (2013) highlight the importance of distinguishing between different type of taxes, estimating large effects of taxes on output in the short run. Zidar (2015) exploits variation in US states to find aggregate effects on employment resulting from tax cuts for lower-income groups (as opposed to tax cuts for the top 10% of the income distributions, which are not found to have a large effect on employment

[10] Barro and Redlick (2011) find that GDP falls 1.1 for each dollar increase in federal taxes, with one year lag.

growth).

Income and wealth inequality have been the object of extensive study in macroeconomics.[11] Piketty and Saez (2003) use a long panel of tax returns to analyse income inequality trends in the US since 1917. The authors find that income inequality, as measured by the share of income by the top decile earners, sharply decreased during World War II and started to increase from the 1970s.[12] Piketty (2014) compiles extensive empirical evidence on the evolution of income and wealth inequality for the US and other countries, finding a noticeable increase in both variables. The author suggests that this increase in inequality is a feature of capitalist economies (given that the rate of return of capital is found to exceed that of economic growth) and advocates for fiscal reforms that establish a global wealth tax and a more progressive income taxation.

In order to understand the causes of wealth inequality, macroeconomic models have relaxed the assumption of a representative agent, allowing for heterogeneity in earnings and other characteristics, in the spirit of Aiyagari (1994).[13] Quadrini and Ríos-Rull (2015) survey the literature on the theories used the explain the causes of inequality, and its implications for the aggregate economy. While economic models predict that wealthy households tend to dissave, this is at odds with the data. De Nardi (2015) surveys the mechanisms that have been used to explain the reasons for wealthy individuals to exhibit a high rate of savings and its implications for wealth inequality. This literature has found that differences in the degree of patience among individuals, the transmission of human capital (skills passed from parents to chil-

[11] Aghion et al. (2015) investigate the relationship between innovativeness and both top-income inequality and social mobility.

[12] Building on the same dataset, Saez and Zucman (2014) capitalise income to produce a measure of wealth inequality, and find that this variable has substantially increased in the last few years.

[13] See Heathcote et al. (2009).

dren) and voluntary bequests across generations or the decisions to become an entrepreneur are plausible reasons that can explain some aspects of the distribution of wealth.

The rest of the chapter is organised as follows. Section 3.2 develops a simple framework to understand the relevant mechanisms behind the effect of taxation and income mobility. Section 3.3 describes how the data regarding income mobility and taxes are constructed. The empirical strategy and the main results are described in Section 3.4. Section 3.5 contains several robustness checks. Further empirical results on the heterogeneous effects of taxes on income mobility are explored in Section 3.6. Lastly, Section 3.7 concludes and discusses potential extensions.

3.2 A Simple Theoretical Framework

Before turning to the empirical analysis, I consider a simple model of labour supply to highlight the key determinants of the relationship between taxation and income mobility.

Consider an economy populated by two households ($i = 1, 2$) with quasi-linear preferences:

$$U(c_{i,t}, n_{i,t}) = \sigma_i c_{i,t} - \frac{X_i}{1+\frac{1}{\eta_i}} n_t^{1+\frac{1}{\eta_i}}$$

where $c_{i,t}$ and $n_{i,t}$ represent consumption and hours worked for household i at date t. Preference parameters can potentially depend on each household characteristics: σ_i represents the relative weight of consumption in the utility function, X_i denotes preferences regarding labour supply (which could be determined by demographic variables, family composition, etc.) and $\eta_i \geq 0$ is the Frisch elasticity of labour supply (a key element in this framework).

Individuals face a budget constraint $c_{i,t} = (w_{i,t}n_{i,t}) - T(w_{i,t}n_{i,t})$, where wages $w_{i,t}$ evolve exogenously following $w_{i,t} = w(1 \pm \varepsilon_{i,t})$, with $\varepsilon_i > 0$. In each period, wages are equal to $w_{1,t} = w(1 + \varepsilon_{1,t})$ for household $i = 1$ and $w_{2,t} = w(1 - \varepsilon_{2,t})$ for household 2 with probability $\pi > 0$. With probability $1 - \pi$ wages become $w_{1,t} = w(1 - \varepsilon_{1,t})$ and $w_{2,t} = w(1 + \varepsilon_{2,t})$. The tax system is assumed to be given as:

$$T'(w_{i,t}n_{i,t}) = \begin{cases} \tau^L + \tau & \text{if } (w_{i,t}n_{i,t}) \geq (1+\varepsilon)wn_{i,t}^{H^*} \\ \tau^L & \text{if } (w_{i,t}n_{i,t}) < (1+\varepsilon)wn_{i,t}^{H^*} \end{cases} \tag{3.1}$$

where τ^L and $\tau^L + \tau$ are the marginal tax rates faced by households with a low or high wage realisation, respectively. $n_{i,t}^{H^*}$ and $n_{i,t}^{L^*}$ are the labour supply functions that result from optimality in consumption-leisure decisions:

$$\begin{aligned} n_{i,t}^{H^*} &= \left((1-\tau^L-\tau)(1+\varepsilon)w\frac{\sigma_i}{X_i}\right)^{\eta_i} \\ n_{i,t}^{L^*} &= \left((1-\tau^L)(1-\varepsilon)w\frac{\sigma_i}{X_i}\right)^{\eta_i} \end{aligned}$$

Assuming that preferences are the same for both type of households ($\eta_1 = \eta_2 = \eta$, $\sigma_1 = \sigma_2 = \sigma$, $X_1 = X_2 = X$), when $\tau = 0$ the tax schedule becomes proportional and the household with a higher realisation of wages (say, $i = 1$) is ranked first in the income distribution:

$$(1-\tau^L)(1+\varepsilon)wn_{1,t}^{H^*} > (1-\tau^L)(1-\varepsilon)wn_{2,t}^{L^*}$$

Conditional on an initial distribution of income, the relative income mobility in this economy is given by $Pr\,(move|w_{2,t-1} = w(1+\varepsilon)) = \pi$.

When τ is positive, the tax schedule is progressive and both labour-supply functions are related by:

$$n_{i,t}^{H^*} = \left(\left(1 - \frac{\tau}{1-\tau^L}\right)\left(\frac{1+\varepsilon}{1-\varepsilon}\right)\right)^{\eta} n_{i,t}^{L^*}$$

In any given period, the optimal labour supply choice weights two opposing effects: (i) a higher wage $\frac{1+\varepsilon}{1-\varepsilon}$ increases the price of leisure and makes the household willing to supply more labour and (ii) a higher rate τ makes the tax system more progressive and reduces the incentives to supply more hours of work. As long as $\tau < (1-\tau_t^L)\frac{2\varepsilon}{1+\varepsilon}$ the household will have incentives to take advantage of a higher wage draw and will optimally choose to supply more labour $n_{i,t}^{H^*} > n_{i,t}^{L^*}$. When τ is high enough, the tax schedule eliminates the incentives to work induced by a a high-wage shock. Particularly, when $\tau = (1-\tau_t^L)\frac{2\varepsilon}{1+\varepsilon}$ the household will decide to not to increase the hours of work due to the wage shock and $n_{i,t}^{H^*} = n_{i,t}^{L^*}$.[14]

In the case of $\tau = (1-\tau_t^L)\frac{2\varepsilon}{1+\varepsilon}$, both households supply the same amount of hours worked. In the presence of preference shocks X_i that counteract the effects of the wage shocks, less progressive taxation would render the labour supply of the households more sensitive to changes in wages, resulting in higher income mobility.

This simple framework allows us to derive the following implications. First and most important, the tax system can reduce income mobility by disincentivizing labour supply. This effect arises because households take less advantage of economic opportunities when the marginal tax rate is very high. The final effect on mobility depends crucially on the Frisch elasticity of labour supply (and whether it is homogeneous across households) since this parameter

[14]When we consider $\tau_L = 0.25$ (approximately the US average federal marginal tax rate on individual income during 1967-1996), $w = 10$ and productivity shocks representing 5% of the base wage w, then we have that the value of τ such that $n_{i,t}^{H^*} = n_{i,t}^{L^*}$ is $\tau = 0.07$, resulting in $\tau^L + \tau = 0.32$.

governs how much the taxes distort the incentives to work. Preferences regarding consumption also matter. A wealth effect on labour supply, which is absent in this minimal framework, will make households more willing to supply work when taxes increase (although this effect will be mitigated by a progressive tax schedule). Another important factor in determining mobility is the wealth accumulation. When savings are allowed, households face an intertemporal optimisation problem. Those households who are lucky and obtain subsequent realisations of high wages will be able to build up savings. The return obtained from these savings will increase total income, making it less likely to move down in the income distribution.

Therefore, while taxes are likely to have an effect on income mobility, the precise impact remains an empirical question. When estimating this effect, it will be important to use data that allows separating household effects (as, for example, taste for leisure) from household shocks. The PSID, given its panel nature, is an attractive dataset to address this question.

3.3 Data and Trends

The PSID (Panel Study of Income Dynamics) is an annual survey elaborated by the University of Michigan since 1968.[15] It follows the same families and their split-offs over time, creating a panel structure. The survey was originally created from two samples: the Survey Research Center (SRC) or core sample (representative at the national level), and the Survey of Economic opportunity (SEO) or Census sample, which over-represents low income households. The PSID provides weights that render the combination of both samples representative of the US population while accounting for the attrition that occurs over

[15]The survey contains data from 1967, since some of the variables asked (e.g. income) refer to the previous year.

time when families are stopped being interviewed.

I restrict the whole sample by considering main adults (the heads and their spouses) of households that are led by a male working head aged 25 to 60 who is not self-employed. Following Aaronson and French (2009), families with a head working less than 300 or more than 4,500 hours per year, earning less than \$3 or more than \$200 per hours in 1996 prices are considered outliers and dropped from the sample. This selection criteria is based on the intention to reflect changes in the income mobility that arise as a result of labour market interaction. In Section 3.5, I check the robustness of the results when considering a more inclusive sample. This selection leads to a total of 5,430 (continuously married) households representing a total of 50,471 observations between 1967 and 1996. The final date of the sample is dictated by the change in the PSID frequency produced in 1997 (referring to data from 1996), when the periodicity of data releases switched from annual to biannual.

3.3.1 Measuring income mobility

This section discusses issues related to income mobility measure and explores its dynamics in the US over the sample period.

Consider an ordering of income in time t in N different ranks (i.e. quantiles of income). Let s_t^n denote the households with income belonging to rank $n \in [1, N]$. The mobility process can be represented by a vector $s_t = (s_t^1, s_t^2, \ldots, s_t^N)$ and a probability matrix P with dimension $n \times n$ and rows adding up to 1 such that:

$$s_t = P_t s_{t-1} \tag{3.2}$$

The vector s_{t-1} summarises the probability distribution of income in period $t-1$. The matrix P characterises the mobility process by determining the probability that a household in income group n at time $t-1$ remains in the same decile next period (entry $P^{n,n}$ in matrix P) or transits to another decile $k \neq n$.[16]

There are different indices that can be used to measure the degree of income mobility.[17] The immobility ratio (IR) summarises changes in relative positions by computing the degree of concentration along the diagonal of matrix P, i.e. the fraction of households that remain in the same income group during two periods of time. In the case of extreme immobility (no household changes deciles between t and $t-1$), IR$=1$.

In a similar vein, the normalised trace index (NTI) proposed by Shorrocks (1978b) uses the elements in the diagonal of P to measure mobility:

$$\text{NTI} = \frac{N - trace(P)}{N-1}$$

When P is the identity matrix, the sum of the diagonal of matrix P is equal to N and the NTI becomes 0.

Both the IR and NTI indices use information from the diagonal of matrix P. The Average Jump Index (Bartholomew (1973)) exploits other information in P to asses the degree of mobility by counting the number of income thresholds (e.g. deciles) that a households passes through between two periods. This index is computed as the average of absolute changes in income ranks for all the sample. A value of 0 indicates perfect immobility (origin independence).

There are other measures that are not restricted to the relative position of households in the income distribution. This is the case of the Pearson

[16]In the special case when vector s_{t-1} contains all the necessary information to predict s_t, i.e. $Prob(s_t|s_{t-1}, s_{t-2,}, \ldots, s_{t-k}) = Prob(s_t|s_{t-1})$ $\forall k \geq 1$ and t, the process s_t is said to be Markovian. P becomes the Markov matrix and transitions along the income distribution between k periods can be obtained from $s_{t+k} = s_t P^k$.

[17]See Fields and Ok (1999) or Jäntti and Jenkins (2015) for exhaustive reviews of the different tools available to measure income mobility.

correlation (r), defined as:

$$r = corr\left(\log(inc_{t-1}), \log(inc_{t)}\right)$$

where inc_t is the real level of income at time t. The Hart index (Hart (1976)) is a variant of this measure and is defined by $H = 1 - r$. When income between two periods is perfectly correlated, we have the case of complete immobility and $H = 0$.

Income is constructed as the Adjusted Aggregate Income (AGI) based on the joint taxable income of the head and spouse in the household.[18] Different measures of income (before taxes, after taxes, and after transfers) are used to assess mobility.

Figure 3.1 plots the evolution of the mobility indices described above using a pre-tax measure of income, setting $N = 10$ (income is divided in deciles) and allowing t to represent a year. While many studies in the literature focus on a longer horizon to analyse mobility (e.g. five years), I choose to use a shorter horizon to be able to identify the effect of taxation on income mobility.

The degree of co-movement between the indices of mobility is high: 1-IR and NTI have a correlation of 95%. The correlation between those two indices and the measure of income ranks passed is of 90 and 88%, respectively. The correlation between the Hart index and the rest of mobility measures ranges between 70 and above 80%.

The evolution of these indices shows a flat profile from the end of the 1960s to the end of the 1970s, although the NTI index exhibits a slightly upward trend during this period. Mobility declines somewhat during the decade of

[18]A broader definition of income would include other sources within the family (e.g. children or other relatives). However this would require making assumptions on how to identify tax units within the household and limit the availability of data. Section 3.5 explores the robustness of the results to different definition of income.

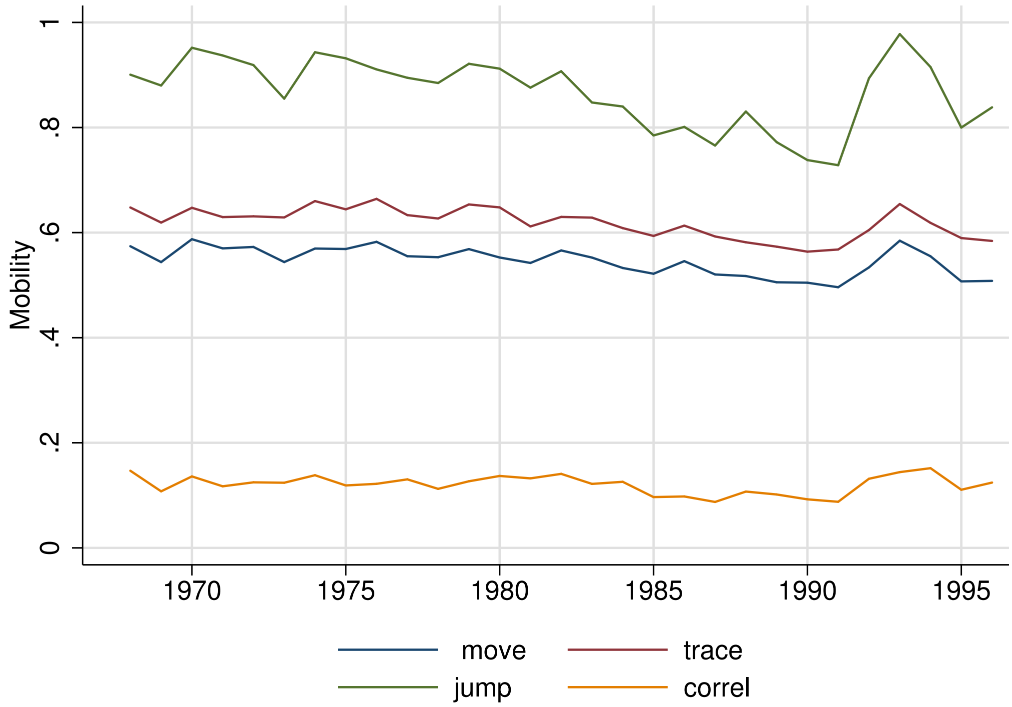
the 80s. It increases more noticeably at the beginning of 1990s (particularly the 1-IR index), but then returns to previous levels towards the end of the sample. The comparison of this evidence with that found in the literature is difficult, since many studies focus on income mobility during a longer time horizon (see for example Hungerford (1993) and those cited in Jäntti and Jenkins (2015)). However, Gittleman and Joyce (1999) considers similar mobility indices for 1, 5 and 10-year windows between 1969 and 1990. The authors find a mild reduction in mobility during the 1970s and an upwards trends until 1990. Gottschalk (1997) reports a transition matrix across quintiles of income between 1973 and 1974 using PSID which is largely similar to my estimation of matrix P in Equation 4.3 for those years (not shown).

While Figure 3.1 displays the probability of mobility overall, it is also interesting to analyse whether these trends in mobility are shared across particular income ranks. Figure A5 shows the evolution of $1 - P^{1,1}$ and $1 - P^{N,N}$ (where $P^{k,k}$ is the k, k element of matrix P in Equation 4.3) for pre-tax and post-tax distributions of income. The probability that a household moves away from the first decile of income (Panel A) has recorded an upward trend during most of the time horizon, only to be reverted towards the end of the sample period. The evolution of the probability of not remaining in the top decile shows a pattern that resembles that of the 1-IR index commented in the previous paragraph: a downwards trend initiated after 1975 which changes direction since the beginning of the 1990s.

3.3.2 Taxation in the US during the sample period

I use the NBER's TAXSIM program to construct the federal tax liabilities faced for each household in the sample. This tax simulator recreates each year's tax law by taking into account features of the US tax code such as the Earned In-

Figure 3.1: Evolution of mobility indices (1967-1996)



Note: Evolution of indices of mobility between 1967-1996. Blue line (move) is 1-IR, i.e. the percentage of people that change income deciles after a year. Red line (trace) is the normalised trace indicator, defined as NTI= $\frac{N-trace(P)}{N-1}$. The green line (jump) represents the average number of income classes (e.g. deciles) than a household goes through after a year. The yellow line (correl) is one minus the absolute correlation of income between two adjacent years. The definition of income is Adjusted Gross Income (AGI) before taxes. The evolution is very similar when using definitions of income after taxes or after taxes and transfers.

come Tax Credit (EITC), the Alternative Minimum Tax (AMT) or deductions and exemption phase-outs. Since TAXSIM only computes state taxes since 1977 and due to the regressive nature of Social Security taxes (FICA), the main empirical results in Section 3.4 make use of a longer horizon (and additional tax reforms) by exclusively considering changes in federal taxation. The effects of including state and social security taxes are explained in Section 3.5.

TAXSIM computes the effective marginal tax rates by increasing taxable income by 1$.[19] For many households in the PSID sample these tax rates are determined by the statutory tax rates associated with each income bracket. However, the effective marginal tax rate of other households will also be determined by the phase-out and other features of the tax code.

Marginal tax rates from TAXSIM are calculated based on tax year, marital status (since the sample only considers (legally) married people, I assume them to file taxes jointly), number of dependants (including those under 17 years), labour income from the head of the household and his spouse, asset income (arising from rentals, dividends or interests), taxable pensions, Social Security Income, property taxes and deductions on mortgage interests.[20]

Federal marginal tax rates have experienced substantial variation during the period considered (1967-1996). Figure 3.2 (Panel A) shows the evolution of the average marginal tax rate for each income decile in US during 1967-1996.[21] The broken line shows the average marginal tax rate for federal individual income taxes from Barro and Redlick (2011).[22] Marginal tax rates show a marked increase during the 1970s, mainly as the result of high inflation that

[19]See Feenberg and Coutts (1993) for an introduction to the TAXSIM program.

[20]Since mortgage interests are not available in the PSID for all the time horizon, I follow Aaronson and French (2009) and assume that 80% of mortgage payments go to interest to impute this variable.

[21]See Figure A6 for the evolution of the average tax rate during the same period.

[22]Barro and Redlick (2011) uses data from a random sample on actual tax files and computes the average marginal tax rate with TAXSIM.

pushed households' income to higher tax brackets because of imperfect indexation of the tax schedule. This upward trend was more substantial for higher incomes: the average marginal tax rate for those in the top decile increased 22 percentage points between 1967 and 1980 (from 26.9% up to 49%) while the increase for the bottom three deciles ranged between 5 and 7 percentage points. This upward trend was substantially reverted during the decade of 1980s. This was the result of major reforms such as Reagan's Tax Reform Act of 1986, which lowered the top statutory rate from 50% to 28% (although the bottom tax rate increased by 4 percentage points). Some smaller tax increases occurred during the early 1990s (e.g. a tax hike to high income earners during Clinton's presidency increased the marginal tax rate of the top decile from 31.3% to almost 33% in 1994).The increase of the average marginal tax rate for the bottom decile since the end of the 1980s and even above the average marginal tax rates of other deciles is the result of the expansion of the EITC.[23]

Panel B in Figure 3.2 shows the individual federal income tax rates for the PSID households computed using the NBER calculator.[24] The figure distinguishes between tax rates before and after the Reagan 1968 reform. The plot shows the noticeable transformation of the tax code following the Tax Reform Act of 1986, which substantially simplified the US tax code.

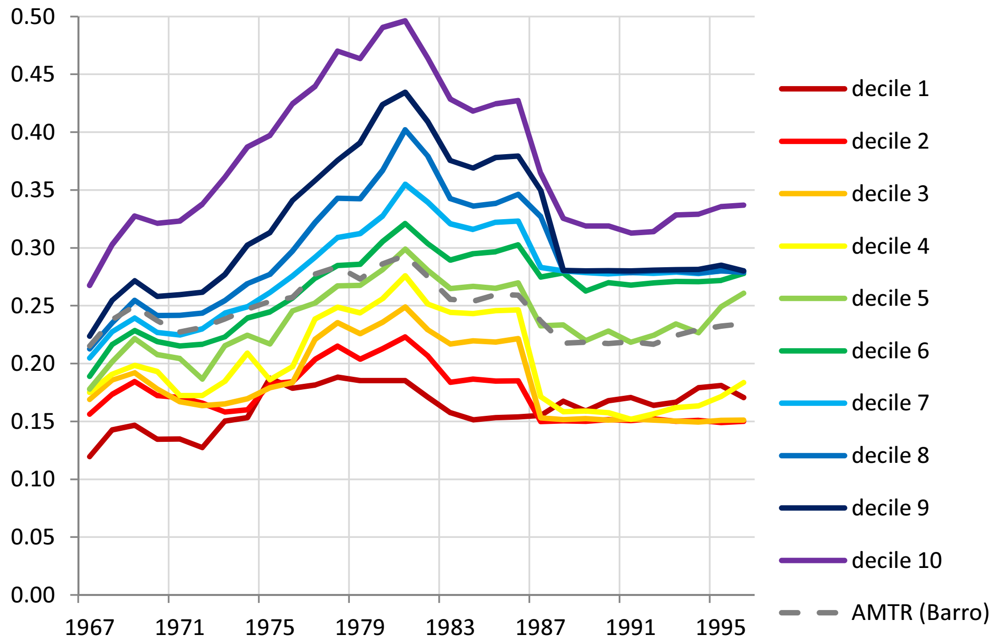
The high variation of taxes over time and across individuals depicted in Figure 3.2 supports the identification of the causal impact of tax reforms on income mobility.

[23]Note that while alterations of the EITC and other provisions have increased the average marginal rate of the bottom deciles, the tax pressure of this group (as measured by the average tax rate shown in Figure A6) has lowered since 1986.

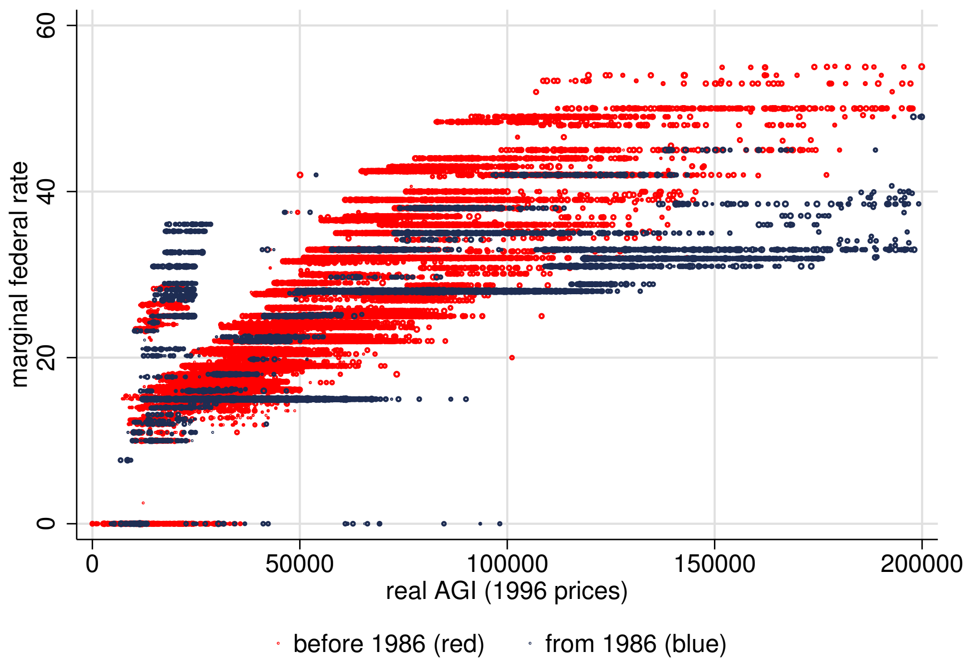
[24]The PSID provided an estimation of the marginal tax rate on federal income during 1976-1991 based on question in the survey regarding exemptions, filling status, etc. The correlation with my marginal tax rate computed through TAXSIM is above 90%. Butrica and Burkhauser (1997) explore the differences between the PSID simulations and TAXSIM.

Figure 3.2: Variation in Marginal Tax Rates (1967-1996)

PANEL A:



PANEL B:



Note: Panel A shows the evolution between 1967-1996 of the average marginal tax rates for each income decile (solid lines) calculated using TAXSIM and PSID data. The dashed line is the economy-wise average marginal tax rate from Barro and Redlick (2011). Panel B shows the relationship between the federal marginal rates on individual income for each household and year in the PSID before and after the 1986 tax reform (in red and blue, respectively) and the real Adjusted Gross Income (in 1996 dollars).

3.3.3 The relationship between taxation and Income mobility

This subsections explores the relationship between income mobility and taxation at the aggregate level. The correlation between the indices of mobility 1-IR and NTI (described in Section 3.3.1) and the AMTR from Barro and Redlick (2011) ranges between 35-45%. However, the evolution of the AMTR is not exclusively restricted to taxes in legislation, but also the result of macroeconomic developments (e.g. inflation increasing the taxable income of households and pushing them to higher tax brackets).

In order to isolate changes in the US tax code from macroeconomic developments, I use the measure of legislated tax changes developed by Romer and Romer (2010). The authors produce a narrative series of changes in federal tax revenues (as a percentage of GDP) by documenting legislated tax changes in the postwar US. Table A1 shows the correlation between the mobility indices mentioned in Section 3.3.1 and the Romer and Romer (2010) measure of legislated tax changes (τ^{Romer}). The relationship between tax changes and mobility appears to be negative, albeit small. An OLS regression of the percentage of households changing deciles (of net income) on the narrative series τ^{Romer} yields a slope coefficient of 0.0159 (robust standard error of 0.0064), suggesting that legislated tax changes that increase tax revenues by 1% of GDP reduces the percentage of households changing deciles by about 1.6%.[25]

The correlations from Table A1 should not be given a causal interpretation. Legislated changes in the tax code are sometimes the result of contemporaneous economic developments, what could result in a problem of endogeneity

[25]The effect of τ^{Romer} on other measures of mobility as described in Section 3.3.1 ranges between -0.0242 and -0.0073 depending on the index considered, the definition of income, and number of income ranks (deciles or quantiles). However, some of these coefficients are estimated with high standard errors.

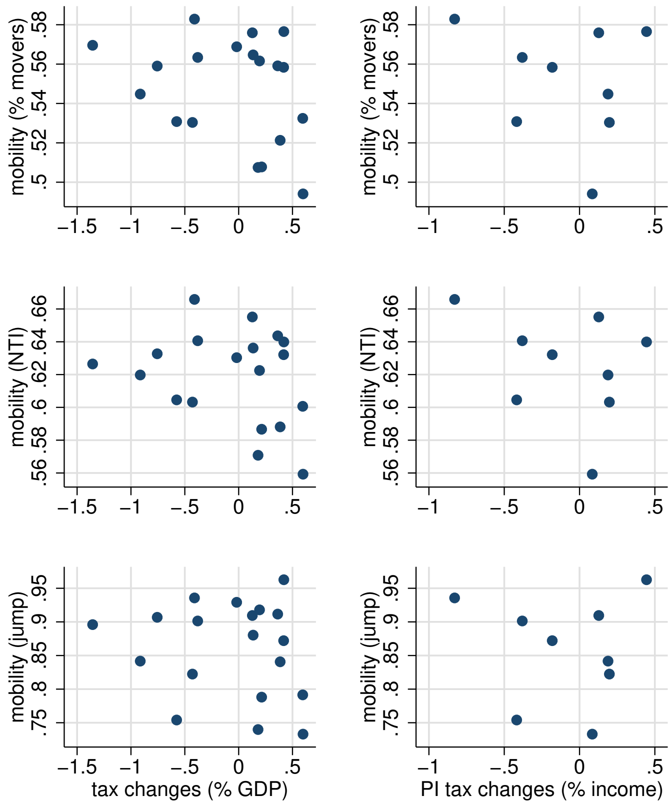
when using aggregate data. In this context, the observed negative correlation between mobility and taxes would be the result of the state of the economy, as opposed to the disincentives produced by the tax system.

To further explore the relationship between income mobility and taxes and the direction of causality, I consider an alternative measure to τ^{Romer} that only includes tax changes not motivated by economic developments. Romer and Romer (2010) produce such narrative by exploring the motivation behind each tax change and classifying them as endogenous (motivated by economic meanings) or exogenous (motivated by ideology or other concerns uncorrelated to the current state of the economy). Mertens and Ravn (2013) and Mertens (2013) further refine this series by considering only those exogenous tax changes that affect employment taxes or individual income that became effective within one year of their legislation.[26]

Figure 3.3 plots the relationship between some relevant indices of mobility based on net income (1-IR, NTI and the number of income thresholds passed) and the two narrative measures of exogenous legislated tax changes ($\tau^{exo-TOT}$ and τ^{exo-PI}) described in the previous paragraph. Large tax cuts seem to be associated with higher values of the mobility indices (more relevant when considering the measure based on unanticipated personal income tax changes, τ^{exo-PI}). Correlations between these two variables range from -11% to -35%. However, the limited number of tax changes meeting the above criteria makes it difficult to obtain conclusive results from this preliminary analysis. The next section analyses further this question by exploiting the disaggregated information contained in the PSID data.

[26] This last criterion accounts for the effect of anticipation (i.e. the case where the econometrician has less information than the economic agents). See Mertens and Ravn (2011) and Mertens and Ravn (2012) for an analysis and evidence on the effects of anticipation in taxes, and Ramey (2011a) for the case of anticipation in government spending.

Figure 3.3: Relationship between taxes (R&R, exogenous) and mobility



Note: Relationship between indices of mobility and tax changes. Graphs in the left column depict the correlation of mobility with the narrative measure of unanticipated exogenous legislated tax changes (as percentage of nominal GDP) from Romer and Romer (2010). Graphs in the right column use a subset of the Romer and Romer series that only considers legislated tax changes that directly affect individual income tax (as a percentage of reported income) from Mertens (2013). Mobility indices are the percentage of people changing income deciles (first row), the normalised trace index (NTI, second row) and the average number of income thresholds passed by a household between two adjacent years.

3.4 Empirical Analysis

3.4.1 Estimation Strategy

The objective of this section is to quantify the effect of taxation on income mobility. To estimate this effect I regress the marginal tax rate on measures of mobility that vary on the definition of income and the number of ranks used to divide the income distribution. I estimate the following regression:

$$mobility_{i,t} = A + B_i + B_t + \beta\tau_{i,t} + \gamma X_{i,t} + \varepsilon_{i,t} \tag{3.3}$$

where $mobility_{i,t} \in \{0, 1\}$ is a binary variable that takes value 1 when a household changes to a different income rank between periods $t-1$ and t. B_i represents individual fixed effects that are assumed to remain constant over time. B_t represents time fixed effects which can have an influence on the dependent variable at the aggregate level (e.g. a macroeconomic shock affecting income mobility). $\tau_{i,t}$ is the marginal tax rate of individual i in time t computed using TAXSIM as explained in Section 3.3. Individual-specific shocks to income mobility in period t are denoted by $\varepsilon_{i,t}$.

The impact of taxes on income mobility can be affected by different factors. Life-cycle considerations are important since the decisions that determine income mobility (labour income or asset income) can be different for younger or older households. Preferences towards leisure can also vary over time, depending on the family composition.[27] Additionally, health-related factors can potentially affect labour income and therefore, mobility.[28] To account for all

[27]Labour supplied by the spouse is an important factor to take into account in this analysis since married female workers have a more disperse distribution of hours worked and are, therefore, more likely to be able to adjust their workload. See Blundell et al. (1998) for an investigation on the effects of tax reforms on female labour supply.

[28]See French (2005) for an investigation on how health affects labour supply and retirement decisions.

these factors, vector $X_{i,t}$ in equation 3.3 includes the age of the head and wife, the size of family, number of children below 18 in the household, a dummy for a working spouse and a dummy for the health status of the head as control variables.

I will consider specifications where the dependent variable $mobility_{i,t}$ differs in how income is measured: income before federal taxes, income after federal taxes but before transfers, and income after taxes and transfers.[29] In this way, we will be able to distinguish whether the potential impact of taxes on income mobility is restricted to the redistributive effect of the tax and transfer system or has a more fundamental reason such as affecting the labour supply choices (as described in Section 3.2). I will also consider specifications where the dependent variable $mobility_{i,t}$ differs on how ranks of the income distribution are defined, distinguishing between deciles and quintiles in order to further support the robustness of the results.

3.4.2 Results from OLS regressions

Table 3.1 shows the results of estimating Equation 3.3 using OLS. The effect of the marginal rate on the probability that the household moves to a different decile of pre-tax income is negative and highly significant: the point estimate is -0.383 (standard error of 0.06).[30] The results are robust to the inclusion of control variables regarding life-cycle, demographics, spouse labour supply or health status.

[29]Transfers include both non-taxable public income (e.g. Supplemental Security Income (SSI) benefits) and income transferred from other sources (e.g. relative). During the period considered, the PSID does not offer exact information on public transfers alone (SSI is reported, but others are not) with yearly frequency. However, the percentage of non-public income in the transfers variable considered here was only about 0.4% in 1980, on average.

[30]Throughout this chapter, models that estimate a binary outcome report estimates that can be interpreted as percentage changes in probability. For example, an estimate of -0.383 represents a -0.383% reduction of success of the dependent variable.

Table 3.1: OLS estimation (with different controls)

	(1) move (D) post-tax	(2) move (D) post-tax	(3) move (D) post-tax	(4) move (D) post-tax	(5) move (D) post-tax
τ_t	-0.383***	-0.381***	-0.390***	-0.402***	-0.401***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
age (H)		-0.578*	-0.618**	-0.620**	-0.622**
		(0.31)	(0.31)	(0.31)	(0.31)
age (W)		0.136	0.134	0.134	0.132
		(0.30)	(0.30)	(0.30)	(0.30)
family size			-0.511	-0.533	-0.535
			(0.66)	(0.65)	(0.65)
num children			-0.308	-0.268	-0.264
			(0.65)	(0.65)	(0.65)
working wife				0.972	0.972
				(0.85)	(0.85)
health status					1.084
					(1.26)
N	50769	50748	50748	50748	50748

Note: OLS estimates of the effects of marginal tax rates on the probability of moving to a different decile of post-tax income. Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

Table 3.2 explores the effects of taxation on alternative measures of income and income ranks. Columns 1-3 report the impact on the probability of changing deciles of income before taxes and transfers, income after taxes and before transfers, and income after taxes and transfers, respectively. Columns 4-6 use the same measures of income but consider instead the effects on the probability of changing quintiles of income. The estimated coefficient of the marginal tax rate is significant above the 99% level for all six specifications. The size of the effect is about -0.40/-0.35 for most regressions, with the specification that considers changes in quantiles of post-transfer income reporting a slightly smaller estimate (-0.285). Overall, these result suggest that there is negative relationship between taxes and income mobility.

3.4.3 Results from IV regressions

The US tax code is progressive and the marginal tax rate depends therefore on income. This causes $\tau_{i,t}$ in Equation 3.3 to be endogenous: when a shock $\varepsilon_{i,t}$ affects income positively, the household will be pushed to a higher tax bracket, and rendering the OLS estimation of Equation 3.3 biased. In principle, the direction of the endogeneity bias is not clear since it depends on how a shock to $\varepsilon_{i,t}$ affects $\tau_{i,t}$. Consider the case of a positive individual shock (e.g. a time varying preference shock) that raises income and, as result of it, mobility. Since the individual will face a higher tax bracket, the relationship between the shock and $\tau_{i,t}$ is positive, making the OLS upward biased. In the opposite case, a shock that decreases income (but still increases mobility) reduces the tax bracket, inducing a downward bias in the OLS estimations. If we consider that positive shocks to $\varepsilon_{i,t}$ are more likely to drive income up (because households have more margin to increase hours worked and income in the face of positive preference shocks as opposed to shocks that make them willing to cut hours

Table 3.2: OLS estimation

	(1) move (D) pre-tax	(2) move (D) post-tax	(3) move (D) post-trans	(4) move (Q) pre-tax	(5) move (Q) post-tax	(6) move (Q) post-trans
τ_t	-0.399***	-0.401***	-0.350***	-0.354***	-0.407***	-0.285***
	(0.06)	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)
age (H)	-0.672**	-0.622**	-0.257	-0.930***	-0.723**	-0.473
	(0.30)	(0.31)	(0.31)	(0.31)	(0.35)	(0.37)
age (W)	0.110	0.132	-0.169	0.503*	0.322	0.130
	(0.30)	(0.30)	(0.30)	(0.30)	(0.34)	(0.36)
family size	-0.316	-0.535	-0.826	-0.173	-0.638	-0.617
	(0.68)	(0.65)	(0.65)	(0.63)	(0.65)	(0.66)
num children	-0.163	-0.264	0.008	-0.589	-0.186	-0.134
	(0.68)	(0.65)	(0.66)	(0.63)	(0.64)	(0.67)
working wife	0.951	0.972	0.542	-0.242	0.187	-0.822
	(0.83)	(0.85)	(0.91)	(0.83)	(0.84)	(0.94)
health status	0.575	1.084	0.340	-1.599	-0.093	-2.843**
	(1.23)	(1.26)	(1.32)	(1.28)	(1.28)	(1.27)
N	50748	50748	47890	50748	50748	47890

Note: OLS estimates of the effects of marginal tax rates on the probability of moving to a different decile (D) or quintile (Q) of income. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

and income), then the first effect dominates, and the correlation between $\varepsilon_{i,t}$ and $\tau_{i,t}$ is positive, making the OLS estimates biased towards positive values.

To address this problem of endogeneity, I construct an instrument that isolates the variation in $\tau_{i,t}$ that is only due to changes in the tax reforms.[31] The instrument is defined as:

$$\Delta\tau_{i,t}^{t-1} = \tau_{i,t}^{t} - \tau_{i,t}^{t-1} \tag{3.4}$$

where $\tau_{i,t}^{t}$ is the actual tax rate faced by household i, with income earned in time t and employing the tax code for fiscal year t. $\tau_{i,t}^{t-1}$ is the counterfactual tax rate that a household i with current income from time t would have faced had the tax schedule from time $t-1$ remained present. Both the actual and the counterfactual tax rates are computed using TAXSIM as described in Section 3.3. When $\Delta\tau_{i,t}^{t-1}$ is positive, a household faces a higher tax pressure as a result of a fiscal reform. Conversely, negative values of $\Delta\tau_{i,t}^{t-1}$ indicates that tax reforms relevant for household i have resulted in lower tax pressure.

Panel A of Figure 3.4 shows the average of $\Delta\tau_{i,t}^{t-1}$ for each income decile. The figure illustrates the extent to which new tax legislation has affected federal income liabilities. As mentioned in Section 3.3, the US has experienced several tax reforms during 1967-1996. These reforms feature prominently in Figure 3.4, showing significant variation over time and across income deciles. These are the cases of, for example the generalised decrease in statutory tax rates of the Tax Reform Act of 1986, or the increase in the rate schedule of high income households as a result of the Omnibus Budget Reconciliation Act of 1993. Panel B of Figure 3.4 shows the percentage of people in the sample that are affected by tax reforms (i.e. those with $\Delta\tau_{i,t}^{t-1} \neq 0$). The figure

[31] This strategy has been also employed in the income elasticity literature: see Gruber and Saez (2002) for an example and Saez et al. (2012) for a review of this literature and its identification approaches.

shows that while some reforms affected most of households (the case of tax legislation during 1980-1989), some tax legislations only targeted low income earners (between 1974-1978), while others were focused on richer households (the case of Omnibus Budget Reconciliation Act of 1993).[32]

Table 4.3 shows the results of estimating Equation 3.3 using tax reforms as an instrument for the marginal tax rate.[33] The estimated coefficients of the marginal tax rates are almost double the size compared to the OLS estimators, suggesting that the latter suffer from an upward bias. The probability of changing to a different decile of income when the tax rate goes up by one percentage point is estimated to be reduced by about 0.8 percentage points (columns 1-3 in Table 4.3): -0.813 when using a pre-tax measure of income or -0.769 when considering income after federal taxes, with a standard error of 0.23.

The effect of taxes on mobility when using a distribution of income ordered in quintiles is also negative, although slightly smaller in magnitude (but bigger in absolute value than the equivalent OLS estimates). The probability that households' income remains in the same quintile after an increase of one percentage point in the marginal tax rate is estimated to be around 0.5% (columns 4-6 in Table 4.3), significant at levels of confidence of 95%.

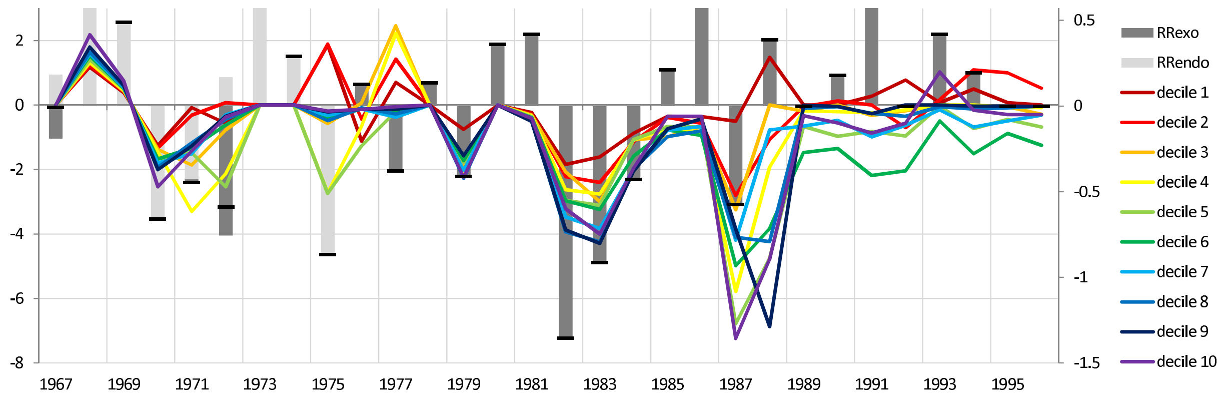
To understand the magnitude of this effect, consider a reduction in the marginal rate of 7 percentage points, which is slightly smaller than the standard deviation of non-zero changes in the actual tax rate $\tau_{i,t}$. The probability that a household moves to a different decile in the income distribution increases by

[32]A systematic correlation between income and changes in tax legislation would threat the validity of $\Delta\tau_{i,t}^{t-1}$ as an exogenous instrument. Including a long panel where tax reforms are the result of different ideological positions mitigates this problem. Section 3.5 checks the robustness of the results to including lag income (see Gruber and Saez (2002) for a discussion).

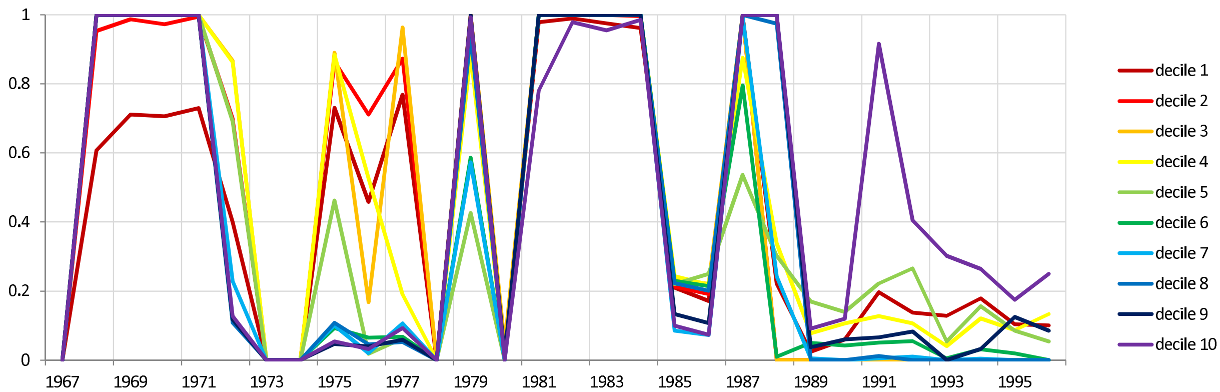
[33]The F-statistic from the first-stage regressions shows a a very high value above 1500 for all the specifications, indicating that the instrument is relevant. Some specifications reduce considerably this value, although it always remains well above 10.

Figure 3.4: The evolution of tax reforms (1967-1996)

PANEL A: Variations in the tax rate due to legislated tax changes



PANEL B: Percentage of households affected by tax reforms



Note: Panel A shows the evolution between 1967 and 1996 of the instrument $\Delta\tau_{i,t}^{t-1} = \tau_{i,t}^{t} - \tau_{i,t}^{t-1}$ (difference between the actual marginal tax rate and a counterfactual marginal tax rate computed using TAXSIM). Grey bars represent the narrative measure of legislated tax changes (as percentage of nominal GDP) from Romer and Romer (2010). These are classified as endogenous tax changes (related to the current state of the economy, in light grey) and exogenous tax changes (unrelated to the state of economy, in dark grey). Panel B shows the (weighted) percentage of people for each decile of income that experience a change in their marginal tax rate in a given year, i.e. $\Delta\tau_{i,t}^{t-1} \neq 0$.

about 5.4-5.7% (depending on the definition of income). Or, in other words, a 7 percentage point cut in the marginal rate makes the household about 5.5% less likely to remain in the same income decile. This represents a tenth of the average likelihood of moving to a different income decile within one year.[34]

The magnitudes are remarkably similar when considering movements across quintiles of income. A 7 percentage point decrease in the marginal rate results in households being around 3.5% more likely to move to a different income quintile. As before, this also represents a tenth of the average probability of movement in the income quintile distribution over the course of a year.

3.4.4 Average and marginal tax rates

Tax reforms can impact on effective marginal rates directly through changes in the statutory tax rates or by introducing provisions that affect deductions, tax credits or coverage. Therefore, while some changes in the US tax code have an effect through the marginal tax rates, others reduce the tax liabilities and affect the average tax rate.

In this subsection I try to isolate the effects of changes in marginal tax rates $\tau_{i,t}$ and average marginal tax rates $\bar{\tau}_{i,t}$, by exploiting variation over time and across individuals in average and marginal tax rates. I estimate a version of Equation 3.3 that includes the average tax rate $\bar{\tau}_{i,t}$.[35] I construct an instrument in the same fashion as in Equation 3.4: I compute the difference between the actual average tax rate in time t and a counterfactual average tax rate based on income obtained in time t taxed using the code of year $t-1$. Figure A7

[34]The average probability of changing deciles between two years is 55% for the pre-tax and post-tax definitions of income, and 57% for income post-transfers. The average likelihood of changing quintiles of income is smaller: 35% for pre-tax and post-tax income and 36% for post-transfer income.

[35]Average tax rates are constructed by dividing federal income liabilities by income. Figure A6 plot the evolution of these tax rates in the US between 1967 and 1996, averaged across income deciles.

Table 3.3: IV estimations

	(1) move (D) pre-tax	(2) move (D) post-tax	(3) move (D) post-trans	(4) move (Q) pre-tax	(5) move (Q) post-tax	(6) move (Q) post-trans
$\tau_{i,t}$	-0.813***	-0.769***	-0.775***	-0.516**	-0.478**	-0.506**
	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)
age (H)	-0.582**	-0.542**	4.481**	-0.895***	-0.708***	2.560
	(0.27)	(0.27)	(1.87)	(0.26)	(0.27)	(1.76)
age (W)	0.078	0.104	-0.198	0.490*	0.317	0.114
	(0.26)	(0.26)	(0.27)	(0.25)	(0.26)	(0.26)
family size	-0.206	-0.437	-0.695	-0.130	-0.619	-0.548
	(0.60)	(0.61)	(0.64)	(0.58)	(0.58)	(0.62)
num children	-0.418	-0.490	-0.261	-0.688	-0.230	-0.274
	(0.61)	(0.62)	(0.65)	(0.59)	(0.59)	(0.62)
working wife	1.927**	1.839*	1.549	0.140	0.355	-0.297
	(0.96)	(0.97)	(0.99)	(0.95)	(0.95)	(0.98)
health status	0.250	0.794	0.028	-1.727	-0.149	-3.006**
	(1.25)	(1.26)	(1.32)	(1.22)	(1.23)	(1.28)
1^{st} stage F-stat	1577	1577	1602	1577	1577	1602
N	50745	50745	47791	50745	50745	47791

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile (D) or quintile (Q) of income. Tax reforms are used as an instrument for the marginal tax rate τ. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

shows the evolution in time and across income deciles of this new instrument.

Table 3.4 displays the results of estimating the impact of marginal tax rates and average tax rates on different income mobility variables. The inclusion of the average tax rate and the use of the new instrument mentioned in the previous paragraph, increase the estimated coefficients on the marginal tax rate. The effect of a percentage point reduction in marginal tax rates fosters relative income mobility across deciles (of pre-tax and post-tax income, columns 1 and 2 in Table 3.4) by about 1% (with a standard errors of 0.18). Similarly, households are about 6% more likely to stay in the same quintile of income when the marginal tax rates goes up by one percentage point (columns 4-6 in Table 3.4).

While the coefficient on marginal tax rate is significant at confidence levels above 99% across all specifications, that of the average marginal tax rate is not. The reported coefficients are high and negative,[36] but none of them are significant at usual significance levels. Therefore, when both marginal and average tax rates are accounted for, only the former has a significant (and negative) effect on the relative mobility.

This evidence suggests that the economic mechanism that determines the effect of taxes on income mobility is based on incentives (the substitution of leisure by labour as shown in Section 3.2) as opposed to the wealth effects originated by a reduction in available income. This view is consistent with Barro and Redlick (2011) and Mertens (2013), who use time series evidence to analyse the impact of tax reforms. This finding has important implications for the design of fiscal policy since reforms that provide more incentives to work are more likely to foster income mobility as opposed to those that only reduce

[36]With the exception of the specification in column 3 (using deciles of post-transfers income), where the estimated coefficient is close to zero but slightly positive.

tax pressure without affecting the marginal tax rate.[37]

3.5 Robustness

This section checks the robustness of previous results along several dimensions. Particularly, I depart from the benchmark estimations by considering alternative definitions of distribution of income, adding further controls the regressions, including state and payroll taxes, checking the stability of the results to samples that differ in time horizon and selection criteria, employing and alternative measure to asses mobility and considering specification with different lags of the explanatory variables. The results from this section contribute to support the evidence of the negative effect of taxes on income mobility.

Alternative definitions of income. The results in Section 3.4 are based on measures of income defined as Adjusted Gross Income (AGI) before taxes, after taxes but before transfers and after taxes and transfers. Tables 3.5 and 3.6 report IV estimates of Equation 3.3 using alternative definitions of income to determine the probability of moving to different ranks. Columns 1 and 2 in Table 3.5 report the estimated effect of marginal tax rates on mobility across deciles of pre-taxes and post-taxes of joint taxable income of head and wife. The coefficients are slightly higher than those in Table 4.3, with IV estimates close to -1 (and standard errors slightly above 0.2) and highly significant.

Columns 3 and 4 of Table 3.5 display the effects on income mobility based on labour income of the head and wife. The point estimation when using pre-tax income is slightly below -1 (-1.06, with standard error of 0.24) while it

[37] As an additional exercise to further support this claim, one could consider episodes of tax reforms that did not affect marginal tax rates but reduced tax liabilities. These episodes are however very scarce and usually much smaller in size, therefore I do not pursue this avenue.

Table 3.4: IV Estimation with average tax rate

	(1) move (D) pre-tax	(2) move (D) post-tax	(3) move (D) post-trans	(4) move (Q) pre-tax	(5) move (Q) post-tax	(6) move (Q) post-trans
τ_t	-1.028***	-0.993***	-0.771***	-0.590***	-0.627***	-0.561***
	(0.18)	(0.18)	(0.16)	(0.15)	(0.16)	(0.15)
$\bar{\tau}_t$	-4.037	-4.209	0.092	-1.391	-2.798	-1.167
	(3.71)	(3.77)	(3.50)	(2.98)	(3.16)	(3.35)
age (H)	-0.077	-0.016	4.462**	-0.721*	-0.358	2.801
	(0.51)	(0.52)	(2.00)	(0.42)	(0.45)	(1.89)
age (W)	-0.151	-0.135	-0.194	0.411	0.158	0.055
	(0.34)	(0.34)	(0.32)	(0.30)	(0.31)	(0.31)
family size	0.465	0.263	-0.710	0.102	-0.154	-0.353
	(0.84)	(0.85)	(0.83)	(0.73)	(0.76)	(0.79)
num children	-4.534	-4.781	-0.170	-2.107	-3.083	-1.430
	(3.73)	(3.79)	(3.42)	(2.98)	(3.16)	(3.27)
working wife	10.105	10.365	1.365	2.959	6.022	2.048
	(7.19)	(7.30)	(6.67)	(5.67)	(6.04)	(6.37)
health status	-1.860	-1.404	0.074	-2.454	-1.611	-3.589*
	(2.22)	(2.24)	(2.08)	(1.86)	(1.95)	(2.00)
1^{st} stage F-stat	944	944	949	944	944	949
N	50745	50745	47791	50745	50745	47791

Note: IV (2SLS) estimates of the effects of marginal and average tax rates on the probability of moving to a different decile (D) or quintile (Q) of income. Tax reforms are used as instruments for the marginal tax rate τ_t and the average tax rate $\bar{\tau}_t$. Specifications also differ on how income is measured (pre-tax AGI and post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

is somewhat smaller when considering post-tax income (-0.74, with standard error of 0.24). Both estimates are significant at confidence levels of 99%.[38]

Column 5 of Table 3.5 report the results when only asset income of the head and wife is used to determine income mobility. The point estimate is, as expected, smaller (-0.402) but significant at levels of 90%. Column 6 considers a broader definition of income that includes other sources of income from other people living in the family.[39] The point estimation is also smaller (-0.472) but significant as well.

Table 3.6 reports the same specifications using the alternative measures of income, but determining mobility in terms of quintiles of income. Results using the taxable income and labour income of head and wife (columns 1-4) are highly significant at levels of 99%. The magnitude of the effects when considering taxable income is about -0.63 and 0.72 (for post-tax and pre-tax income respectively, with standard errors of 0.23), and higher when considering labour income (-1.03 for pre-tax income and -0.88 for post-tax income, with same standard errors). The estimated coefficients when considering asset income or (adjusted) family income (columns 5 and 6 of Table 3.6 respectively) are negative but small and not significant.

Further controls. The benchmark estimates control for a number of life cycle and demographic factors. Section 3.2 pointed out that accumulated wealth could reduce mobility since households with higher asset income are less likely to move down the income distribution when labour income is lower. Since information on wealth is not measured frequently in the PSID during the pe-

[38]Interestingly, the estimated coefficient on the dummy variable for working wife in the household become larger and more significant than in other specifications.

[39]This measure of income is divided by the square root of the number of people living in the family to adjust it for family size. See Jäntti and Jenkins (2015).

Table 3.5: Robustness to alternative definitions of income (deciles)

	(1) tax inc. (pre-tax)	(2) tax inc. (post-tax)	(3) labour inc. (pre-tax)	(4) labour inc. (post-tax)	(5) asset inc. (pre-tax)	(6) fam. inc. (pre-tax)
$\tau_{i,tt}$	-0.979***	-0.939***	-1.065***	-0.742***	-0.402*	-0.473**
	(0.23)	(0.24)	(0.23)	(0.24)	(0.24)	(0.23)
age (H)	-0.775***	-0.418	-0.684***	-0.640**	-0.692***	1.410***
	(0.27)	(0.27)	(0.26)	(0.27)	(0.27)	(0.26)
age (W)	0.298	-0.013	0.170	0.243	0.475*	-0.128
	(0.26)	(0.26)	(0.25)	(0.26)	(0.26)	(0.25)
family size	-0.756	-0.572	-0.304	-0.091	3.891***	0.560
	(0.61)	(0.61)	(0.61)	(0.62)	(0.61)	(0.59)
num children	-0.528	-0.492	-0.991	-1.397**	-5.090***	-1.752***
	(0.62)	(0.62)	(0.62)	(0.62)	(0.62)	(0.60)
working wife	2.271**	2.350**	3.857***	2.944***	0.813	1.158
	(0.96)	(0.97)	(0.97)	(0.97)	(0.97)	(0.93)
health stastus	1.678	0.865	0.767	0.644	3.081**	-0.324
	(1.25)	(1.27)	(1.26)	(1.27)	(1.28)	(1.23)
1^{st} stage F-stat	1577	1577	1577	1577	1577	1577
N	50745	50745	50745	50745	50745	50745

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile of income. Tax reforms are used as an instrument for the marginal tax rate τ. Specifications differ in how income is defined: columns 1-2 refer to taxable income of head and wife, columns 3-4 refer to labour income of head and wife, column 5 refers to asset income of head and wife, and column 6 refers to adjusted family income (the adjustment consists on dividing family income by the square root of family size). Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

Table 3.6: Robustness to alternative definitions of income (quintiles)

	(1) tax inc. (pre-tax)	(2) tax inc. (post-tax)	(3) labour inc. (pre-tax)	(4) labour inc. (post-tax)	(5) asset inc. (pre-tax)	(6) fam. inc. (pre-tax)
τ_t	-0.722***	-0.628***	-1.027***	-0.876***	-0.115	-0.288
	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.22)
age (H)	-0.739***	-0.675**	-0.379	-0.534**	-0.498*	1.092***
	(0.26)	(0.27)	(0.26)	(0.26)	(0.26)	(0.25)
age (W)	0.358	0.311	-0.057	0.080	0.315	-0.017
	(0.25)	(0.26)	(0.24)	(0.24)	(0.25)	(0.23)
family size	-0.520	-0.856	-1.007*	-0.274	3.798***	0.107
	(0.58)	(0.58)	(0.58)	(0.58)	(0.61)	(0.55)
num children	-0.640	-0.072	-0.260	-0.983*	-5.079***	-0.761
	(0.59)	(0.59)	(0.59)	(0.59)	(0.61)	(0.56)
working wife	0.762	0.948	2.991***	2.589***	-1.385	-0.228
	(0.95)	(0.95)	(0.94)	(0.95)	(0.95)	(0.87)
health status	-0.476	-0.840	0.317	-0.459	1.796	-0.064
	(1.22)	(1.22)	(1.22)	(1.22)	(1.24)	(1.15)
1^{st} stage F-stat	1577	1577	1577	1577	1577	1577
N	50745	50745	50745	50745	50745	50745

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different quintile of income. Tax reforms are used as an instrument for the marginal tax rate τ. Specifications differ in how income is defined: columns 1-2 refer to taxable income of head and wife, columns 3-4 refer to labour income of head and wife, column 5 refers to asset income of head and wife, and column 6 refers to adjusted family income (the adjustment consists on dividing family income by the square root of family size). Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

riod considered,[40] I use net home equity (self-reported house value minus the remaining mortgage on the house) to proxy for net worth.[41] Columns 1-3 in Table 3.7 report the IV estimates of the marginal tax rate on income mobility when including wealth as a control. The coefficient of this variable (measured in thousands of 1996 dollars) is negative as expected: an increase of 100,000 in house equity increases the probability of staying in the same income decile by about 5%. The estimated coefficient of the marginal tax rate is close to -0.8 for all three specifications considered (which vary in how income is measured) and remains highly significant (at levels of 99%).

Next, I consider dummy variables of the position in the income distribution in year $t-1$ as controls. This aims to take into account two potential issues. First, the previous position in the income distribution can be informative of the likelihood of moving to other income rank. And second, related to the previous point, people positioned in the first or last income rank (e.g. the 1st and 10th decile) are, by definition, less likely to move (since their movements are restricted to one direction). Columns 4-6 of in Table 3.7 shows the results of including these new controls. The new variables have large and positive estimated coefficients,[42] which seem to suggest that households with income belonging to the central part of the distribution (deciles 4-7) are more likely to experience movements along the income ranks. Regarding the estimated coefficients on the marginal tax rates, controlling for the previous position in the income distribution reduces the impact of marginal tax rates slightly (by about 0.1 percentage points), but the coefficients remain significant at

[40] PSID data only includes snapshots of wealth for years 1984, 1989 and 1994, and from 1999 onwards.

[41] Information on household equity is included yearly in the PSID, with the exception of years 1973 and 1974. Fairlie and Krashinsky (2012) reports that net home equity represents 60% of the average homeowner wealth (64% for the median homeowner).

[42] This specification highlights a common problem with linear probability models: the sum of the estimated coefficients can be in excess of 100, which is not conceptually possible.

confidence levels of 99%.

As discussed in Section 3.4, a systematic relation between the instrument $\Delta\tau_{i,t}^{t-1}$ and previous income levels can lead to biased estimations if the error term $\varepsilon_{i,t}$ in Equation 3.3 also depends on previous income. To address this issue, columns 1-3 in Table 3.8 report the effect of marginal tax rates on the probability of income mobility when controlling for previous income (measured by AGI). The inclusion of the variable supports the validity of the instrument while also controls for non-labour income (e.g. asset income). The estimated coefficients are not noticeably changed with respect to the main results (see Table 4.3), and remain in the region of -0.8 (standard errors of 0.22) and statistically significant at levels of confidence of 99%.

Lastly, columns 4-6 in Table 3.8 include absolute changes in income (AGI) as an additional control. By definition of the mobility variables (which capture the probability that income in period t belongs to a different income rank than that of period $t-1$), this variable explain most of the likelihood of relative income movements. The inclusion of this variable strengthens the effect of marginal tax rate on income mobility by about 0.2 percentage points: the estimated coefficients become close to -1 (with standard errors of 0.22), while remaining significant.

State and Payroll tax rates. So far, only federal income tax liabilities have been considered in the analysis. However, the total effective tax pressure in the US also includes payroll tax liabilities (FICA) and state-level tax liabilities. Payroll taxes are charged at the federal level to both employees and employers in order to fund social benefits programs (Social Security and Medicare). The FICA marginal tax rate has been relatively low until 1979 with substantially

Table 3.7: Robustness to further controls

	(1) move (D) pre-tax	(2) move (D) post-tax	(3) move (D) post-trans	(4) move (D) pre-tax	(5) move (D) post-tax	(6) move (D) post-trans
τ_t	-0.828***	-0.789***	-0.795***	-0.695***	-0.648***	-0.655***
	(0.23)	(0.24)	(0.23)	(0.23)	(0.23)	(0.23)
age (H)	-0.461	-0.436	4.533**	-0.444*	-0.403	4.512**
	(0.28)	(0.28)	(1.89)	(0.26)	(0.27)	(1.85)
age (W)	0.114	0.178	-0.099	-0.022	0.002	-0.320
	(0.27)	(0.27)	(0.28)	(0.25)	(0.26)	(0.27)
family size	0.018	-0.314	-0.578	-0.305	-0.543	-0.819
	(0.64)	(0.64)	(0.68)	(0.60)	(0.60)	(0.63)
num children	-0.530	-0.473	-0.115	-0.365	-0.435	-0.207
	(0.64)	(0.65)	(0.69)	(0.61)	(0.61)	(0.64)
working wife	1.964**	2.315**	1.633	1.612*	1.506	1.131
	(0.99)	(0.99)	(1.02)	(0.93)	(0.94)	(0.96)
health status	0.290	1.232	0.468	0.116	0.660	-0.071
	(1.29)	(1.29)	(1.36)	(1.24)	(1.25)	(1.31)
wealth	-0.045***	-0.051***	-0.044***			
	(0.01)	(0.01)	(0.01)			
$decile_{t-1} = 2$				13.310***	13.321***	13.297***
				(1.39)	(1.38)	(1.40)
$decile_{t-1} = 3$				18.715***	18.945***	17.955***
				(1.42)	(1.42)	(1.45)
$decile_{t-1} = 4$				22.120***	22.462***	22.363***
				(1.38)	(1.38)	(1.42)
$decile_{t-1} = 9$				11.684***	11.852***	12.171***
				(1.40)	(1.39)	(1.44)
1^{st} stage F-stat	1570	1570	1597	1567	1567	1596
N	47508	47508	44547	50745	50745	47791

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile of income. Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Variable *wealth* is measured in thousands of 1996 dollars. $decile_{t-1} = k$ is a dummy variable equal to 1 if the income decile in the previous period was k. Rows for deciles 5 to 8 are omitted in the interest of space (all coefficients are significant) Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

Table 3.8: Robustness to income controls

	(1) move (D) pre-tax	(2) move (D) post-tax	(3) move (D) post-trans	(4) move (D) pre-tax	(5) move (D) post-tax	(6) move (D) post-trans
τ_t	-0.800***	-0.762***	-0.771***	-1.053***	-1.009***	-0.983***
	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)
age (H)	-0.714**	-0.615**	4.466**	-0.437*	-0.397	3.568**
	(0.28)	(0.28)	(1.88)	(0.25)	(0.25)	(1.75)
age (W)	0.089	0.110	-0.195	0.039	0.065	-0.208
	(0.26)	(0.26)	(0.27)	(0.24)	(0.24)	(0.26)
family size	-0.253	-0.464	-0.712	0.046	-0.184	-0.441
	(0.60)	(0.61)	(0.63)	(0.57)	(0.58)	(0.62)
num children	-0.402	-0.481	-0.255	-0.413	-0.486	-0.274
	(0.61)	(0.62)	(0.65)	(0.58)	(0.59)	(0.63)
working wife	1.771**	1.753*	1.500	2.940***	2.855***	2.410**
	(0.90)	(0.90)	(0.93)	(0.91)	(0.91)	(0.94)
health status	0.321	0.834	0.048	-1.114	-0.573	-1.079
	(1.24)	(1.25)	(1.32)	(1.18)	(1.19)	(1.27)
log $income_{t-1}$	1.649	0.906	0.552			
	(1.66)	(1.68)	(1.69)			
abs ($\Delta income_t$)				94.612***	94.837***	87.523***
				(2.22)	(2.22)	(2.00)
1^{st} stage F-stat	1791	1791	1802	1581	1581	1605
N	50741	50741	47788	50736	50736	47783

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile of income. Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Additional controls account for lagged income and absolute income changes, where income is measured as household's AGI. Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

less variation than federal income taxes.[43] On the other hand, TAXSIM can only compute marginal tax rates at the state level from 1977.

To check the robustness of the results to the inclusion of payroll and state tax liabilities, I compute the marginal and average tax rate on total tax liabilities (federal income, FICA and sate) from 1978 to 1996 using TAXSIM.[44] The number of available observations in the PSID sample is reduced by about a third, down to about 35,400. Table 3.9 shows the estimated coefficients of the marginal tax rate on the probability of changing deciles (columns 1-3) and quintiles (columns 4-6) of income. The estimated coefficients are lower by about 0.25 percentage points when compared to those in Table 4.3, but still significant across all specifications considered at confidence levels of at least 90%. When considering pretax income, household are 0.565% (standard error of 0.20) less likely to move to a different income decile when the marginal tax rate goes up by one percentage point. Specifications considering transition across income quantiles report estimated coefficients ranging from -0.38 to -0.33 (with standard errors of 0.19 and 0.20).

Sample stability. The Tax Reform Act of 1986 had a substantial impact on the US tax code in many different dimensions (e.g. significant cuts in statutory tax rates, elimination of several provisions). To account for potential sample instability in the estimations due to this major reform, Table 3.10 reports the coefficients of marginal tax rates and average tax rates on the probability of moving to a different decile of income considering a sample before 1986 (columns 1-3) and from 1986 onwards (columns 4-6). The differences in

[43] FICA marginal tax rate has averaged 0.03% between 1967 and 1978. Its standard deviation between 1967-1996 is half of the federal income tax rates, and about a third of it during 1967-1997. See Barro and Redlick (2011).

[44] Figure A8 plots the variation across individuals in total marginal tax rates during this period.

Table 3.9: IV Estimation with State and Payroll Tax rates

	(1) move (D) pre-tax	(2) move (D) post-tax	(3) move (D) post-trans	(4) move (Q) pre-tax	(5) move (Q) post-tax	(6) move (Q) post-trans
τ_t	-0.565***	-0.451**	-0.334*	-0.379*	-0.356*	-0.335*
	(0.20)	(0.21)	(0.20)	(0.19)	(0.20)	(0.19)
$\bar{\tau}_t$	1.820	5.246	1.777	0.519	3.168	1.137
	(4.19)	(5.03)	(4.28)	(3.87)	(4.32)	(3.96)
age (H)	0.079	-0.323	-0.313	-0.521	-0.097	-0.240
	(0.43)	(0.49)	(0.44)	(0.41)	(0.45)	(0.41)
age (W)	-0.678	-0.317	-0.101	0.263	-0.324	0.018
	(0.42)	(0.48)	(0.43)	(0.40)	(0.45)	(0.40)
family size	-1.385	-1.997	-2.023	-0.726	-1.341	-0.940
	(1.34)	(1.53)	(1.35)	(1.25)	(1.36)	(1.27)
num children	2.093	5.196	2.366	0.563	3.253	1.261
	(5.06)	(6.03)	(5.15)	(4.69)	(5.20)	(4.77)
working wife	-3.635	-11.042	-4.178	-1.522	-8.103	-3.984
	(10.10)	(11.99)	(10.29)	(9.37)	(10.37)	(9.57)
health status	2.007	3.240	2.657	-1.541	1.737	-0.342
	(2.56)	(2.91)	(2.58)	(2.41)	(2.61)	(2.45)
1^{st} stage F-stat	1416	1416	1412	1416	1416	1412
N	35408	35408	35374	35408	35408	35374

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile (D) or quintile (Q) of income. Taxes include federal income, payroll and state liabilities. Tax reforms are used as instruments for the marginal tax rate τ_t and the average tax rate $\bar{\tau}_t$. Specifications also differ on how income is measured (pre-tax AGI and post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

the coefficients before and after are not statistically different from each other (probably because of the higher standard errors resulting from lower sample size). For example, the estimated coefficient of the marginal tax rate is -0.835 (standard error of 0.41) considering pre-tax income before 1986, and -1.018 (standard error of 0.26) after 1986. All the estimated coefficients are statistically significantly different from 0 at confidence levels of 99%.

The estimated coefficients do however show differences between before and after 1986. While the coefficients are always negative, they are not significant after 1986 (in line with the results from Table 3.4) and significant across some specifications (using pre-tax and post-tax income) before 1986.This could suggest that the effect of average tax rates diminishes when taxes and, most noticeably, transfers are introduced (before 1986). However, a counterfactual analysis of tax reforms would be required to add support to this interpretation.

Sample selection. The PSID sample selected for this chapter responds to the goal of targeting households that are actively involved in the labour market. I now check whether the results presented in Section 3.4 are robust to different sample specifications.

As described in Section 3.3, PSID includes two subsamples: a representative sample of the US (core or SRC sample) and a sub-sample that over-represents low-income (the Survey of Economic Opportunity, SEO, a project from which PSID was originated). To insure representability, the PSID provides weights to account for different sampling probabilities and attrition. Columns 1 and 2 of Table 3.11 presents results when only the core sample (and no weights) are used. This represents a reduction of almost 40% in the sample size. However, the estimated effect of marginal tax rates on the probability of changing income deciles is still negative and highly significant at 99%: point estimates of -0.95

Table 3.10: IV Estimation Sample Stability

	(1) pre 1986 pre-tax	(2) pre 1986 post-tax	(3) pre 1986 post-trans	(4) post 1986 pre-tax	(5) post 1986 post-tax	(6) post 1986 post-trans
τ_t	-0.835**	-0.971***	-0.847**	-1.018***	-0.837***	-0.690***
	(0.41)	(0.37)	(0.35)	(0.26)	(0.27)	(0.26)
$\bar{\tau}_t$	-5.145***	-2.908*	-1.476	-0.959	-0.068	-0.867
	(1.76)	(1.64)	(1.53)	(1.91)	(1.92)	(1.91)
age (H)	1.229*	0.628	0.003	0.322	-1.073	-0.858
	(0.69)	(0.64)	(2.08)	(0.90)	(0.94)	(0.95)
age (W)	-0.164	-0.005	-0.182	-0.745	0.760	0.837
	(0.43)	(0.42)	(0.45)	(0.89)	(0.92)	(0.94)
family size	-0.802	-0.340	-0.740	-1.094	-1.274	-1.062
	(0.81)	(0.80)	(0.85)	(1.37)	(1.38)	(1.35)
num children	-5.072***	-3.300*	-1.710	-0.147	0.222	-0.342
	(1.86)	(1.73)	(1.58)	(2.40)	(2.41)	(2.38)
working wife	9.666***	5.382*	3.978	3.410	1.902	0.441
	(3.11)	(2.84)	(2.59)	(4.62)	(4.66)	(4.64)
health status	-2.792	-0.544	-2.121	-1.544	1.754	1.853
	(1.97)	(1.91)	(1.98)	(2.41)	(2.34)	(2.41)
1^{st} st. F-stat	157	157	158	813	813	810
N	29069	29069	26144	19356	19356	19321

Note: IV (2SLS) estimates of the effects of marginal and average tax rates on the probability of moving to a different decile of income before and after (including) 1986. Tax reforms are used as instruments for the marginal tax rate τ_t and the average tax rate $\bar{\tau}_t$. Specifications also differ on how income is measured (pre-tax AGI and post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

and -0.72 (standard errors of 0.25) when considering pre-tax and post-tax income respectively.

Columns 3 and 4 of Table 3.11 display the estimations based on a sample that additionally includes households with a head younger than 25 or older than 60 years.[45] The estimated coefficients of the marginal tax rates are smaller (but still significant at confidence levels of 90%), suggesting that the income mobility of very young or old households is not as much determined by changes in taxation compared to households with a head aged 25-60.

Next, I consider whether the benchmark sample criteria may induce a bias due to households being self selected into groups. These would be the case if higher taxes affect the decision of work at the extensive margin (a head of household decides to become unemployed when taxes go up) or to become self-employed.[46] To address this, columns 3 and 4 of Table 3.11 report the estimates when the sample is extended to include households with a self-employed status. The point estimates of the effect of marginal tax rates remain similar (and highly significant) to the benchmark estimations: -0.88 (standard error of 0.20) and -0.84 (standard error of 0.21) when considering pre-tax and post-tax income, respectively.

Alternatively, columns 5 and 6 display the estimates when the sample also includes households whose head is unemployed.[47] Marginal tax rates are estimated to reduce income mobility by 0.72 and 0.74% (for pre-tax and post-tax income, respectively; standard errors of 0.22). These coefficients are statistically significant at confidence levels of 99%.

Lastly, I extend the sample to include families whose head is a female.[48]

[45]These thresholds are often considered to determine the prime age for labour market engagement. See for example Keane (2011).

[46]The potential effect of taxes on the probability of becoming an entrepreneur is further discussed in Section 3.7.

[47]A dummy for heads who are employed is added to these specifications.

[48]PSID usually assigns the role of the head of the household to a male when he is present.

Columns 9 and 11 show the estimates when considering this enlarged sample (these include a dummy variable for male heads). The estimated coefficients are quantitatively similar to the benchmark results, and marginal taxes are found to increase the probability of households remaining in the same pre-tax income decile by about 0.6% (0.8% for post-tax income, standard errors of 0.23 and 0.24, respectively).

Alternative dependent variable. The dependent variable $mobility_{i,t}$ used in the main results exploits the information in the diagonal of the probability matrix P in Equation 4.3: it computes the probability that a household with income belonging to rank k in period $t-1$ remains in the same position in time t. An alternative way to measure mobility is to calculate the number of income ranks that a household crosses when moving in the income distribution. For example, this new variable, $jump_{i,t}$, takes value of 3 if a household moves in the income distribution from income decile k in time $t-1$ to income rank $k+3$ or $k-3$ in period t. Hence, this allows to analyse mobility by effectively using information in the rest of the cells in matrix P apart from those in its diagonal.[49]

Table 3.12 reproduces the main results of Table 4.3 but switching the dependent variable $mobility_{i,t}$ by the newly created measure of mobility $jump_{i,t}$.[50] A cut in the marginal tax rate of 1 percentage point increases the average number of income deciles that a household would cross while moving in the pre-tax income distribution by 0.013 (standard error of 0.001, column 1 of Table 3.12).

But in some, occasions this role corresponds to the wife (e.g. when the female prefers to be designated as the head).

[49]The average of variable $jump_{i,t}$ in the sample is 0.89. The average number of income ranks crossed by those who move in the income distribution is 1.61.

[50]Estimations in Table 3.12 also include the average tax rate as an explanatory variable. The estimated coefficients on the marginal tax rates remain quantitatively the same when average tax rate is not included, but are estimated with higher standard errors, reducing their statistical significance.

Table 3.11: Robustness to sample selection

	(1) core pre-tax	(2) core post-tax	(3) all ages pre-tax	(4) all ages post-tax	(5) self-emp. pre-tax	(6) self-emp. post-tax	(7) unemp. pre-tax	(8) unemp. post-tax	(9) female pre-tax	(10) female post-tax
τ_t	-0.719***	-0.953***	-0.449**	-0.417*	-0.877***	-0.839***	-0.721***	-0.740***	-0.759***	-0.804***
	(0.25)	(0.25)	(0.21)	(0.22)	(0.20)	(0.21)	(0.22)	(0.22)	(0.24)	(0.23)
age (H)	-0.730**	-0.735**	-0.565**	-0.571**	-0.295	-0.197	-0.553**	-0.447*	-0.576**	-0.531**
	(0.29)	(0.29)	(0.23)	(0.23)	(0.22)	(0.22)	(0.27)	(0.27)	(0.27)	(0.27)
age (W)	0.278	0.267	0.039	0.129	-0.048	-0.171	0.076	0.048	0.033	0.116
	(0.28)	(0.28)	(0.22)	(0.22)	(0.21)	(0.21)	(0.26)	(0.26)	(0.26)	(0.26)
family size	-0.731	-0.455	0.206	-1.000*	-0.825	-0.591	0.031	-0.532	-0.209	-0.575
	(0.66)	(0.67)	(0.57)	(0.58)	(0.54)	(0.54)	(0.60)	(0.61)	(0.61)	(0.61)
num children	-0.292	-0.848	-0.956	0.206	-0.209	-0.191	-0.628	-0.515	-0.488	-0.390
	(0.68)	(0.68)	(0.58)	(0.59)	(0.55)	(0.55)	(0.61)	(0.61)	(0.62)	(0.62)
health status	0.597	-0.764	2.476**	2.881**	-0.505	-0.416	1.165	1.129	0.746	0.792
	(1.35)	(1.36)	(1.14)	(1.15)	(1.11)	(1.11)	(1.24)	(1.25)	(1.25)	(1.26)
working wife	2.730***	2.982***	1.674*	3.176***	1.238	0.787	2.110**	1.569*	1.934**	1.520
	(0.99)	(0.99)	(0.89)	(0.89)	(0.84)	(0.84)	(0.94)	(0.94)	(0.96)	(0.97)
working head							-10.210	-9.993		
							(6.96)	(6.91)		
male head									-27.764	-29.135
									(22.27)	(22.37)
1^{st} st. F-stat	1392	1392	1650	1650	1321	1321	1192	1192	1576	1576
N	32090	32090	60253	60253	60231	60231	51620	51620	50768	50768

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile of income. Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI or post-tax AGI) and on alterations of the benchmark sample from Table 4.3. Columns 1 and 2 drops the households from the SEO subsample (which over-represents low-income families) and uses equal weights for all observations. Columns 3 and 4 expands the sample to include households with a working head from all ages (not only between 25-60 years). Columns 5 and 6 add households with a head who is self-employed. Columns 7 and 8 also incorporate households with an unemployed head, including an additional control (*working head*, a dummy for employed heads of households). Columns 9 and 10 extend the benchmark sample with households that also have a female head (a dummy variable for male heads is also included). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

The estimated coefficient when considering a post-tax income distribution is very similar (point estimate of -0.012, standard error of 0.01, column 2) and slightly smaller (point estimate of -0.008, column 3) when considering a post-transfers income distribution.

Results are, as expected, reduced by half when the number of ranks are lowered from 10 (deciles) to 5 (quintiles). Columns 4-6 of Table 3.12 report these results, with point estimates between -0.005 and -0.006 depending on the measure of income considered. All the coefficients of the marginal tax rates on Table 3.12 are significant at confidence levels of 95%.

Dynamic specifications. Following the model described in Section 3.2, the effect of taxation on the probability of income mobility is determined in the labour market, which is the result of a static optimisation problem. There are however reasons for believing the idea that this effect could have some dynamic structure. For example, decisions on changes in asset income as a result of variation in taxes may take more than a period to take effect (since wealth accumulation is the result of an inter-temporal problem).

To account for these effects, I estimate versions of Equation 3.3 that differ in the dynamic effect of the marginal tax rate $\tau_{i,t}$ on the probability of income mobility. Table 3.13 (columns 1 and 2) reports the estimated coefficients of the marginal tax rate when its effect is assumed to be lagged one period. The point estimates (-0.57 and -0.41 for pre-tax and post-tax income specifications; standard errors of 0.24) are smaller although still significantly different from zero (at levels of confidence of 90 and 95%). When the tax rate is lagged two periods (columns 3 and 4), the effect is positive but insignificant when considering pre-tax income, and positive and only marginally significant when

Table 3.12: IV Estimation Jumps with ATR

	(1) jump (D) pre-tax	(2) jump (D) post-tax	(3) jump (D) post-trans	(4) jump (Q) pre-tax	(5) jump (Q) post-tax	(6) jump (Q) post-trans
τ_t	-0.013***	-0.012***	-0.008**	-0.006***	-0.006***	-0.005**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\bar{\tau}_t$	-0.112	-0.126	-0.002	-0.045	-0.062	-0.029
	(0.08)	(0.09)	(0.08)	(0.04)	(0.05)	(0.05)
age (H)	-0.000	0.002	0.067	-0.005	-0.000	0.042
	(0.01)	(0.01)	(0.05)	(0.01)	(0.01)	(0.03)
age (W)	0.001	-0.001	0.005	0.004	0.001	0.001
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
family size	0.016	0.014	-0.006	0.002	0.001	-0.005
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
num children	-0.139*	-0.148*	-0.022	-0.055	-0.067	-0.032
	(0.08)	(0.09)	(0.08)	(0.04)	(0.05)	(0.04)
working wife	0.174	0.202	-0.060	0.068	0.106	0.024
	(0.16)	(0.17)	(0.15)	(0.08)	(0.09)	(0.09)
health status	-0.042	-0.041	-0.016	-0.037	-0.025	-0.047*
	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)
1^{st} stage F-stat	944	944	948	944	944	948
N	50745	50745	47742	50745	50745	47742

Note: IV (2SLS) estimates of the effects of marginal tax rates on the number of income ranks (in deciles, D, or quintiles, Q) crossed along the income distribution. Tax reforms are used as instruments for the marginal tax rate τ_t and the average tax rate $\bar{\tau}_t$. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

considering post-tax income.[51] Further lags of the tax rate results on negative but insignificant coefficients: columns 5 and 6 report the estimates for τ_{t-3}. Lags beyond 3 remain negative but are usually insignificant (not reported). These results suggest that the effect of taxes on income mobility is most noticeable on impact and during the following year. I do not find significant evidence on the effect of tax reforms on income mobility beyond that time.

Table 3.13 replicates the robustness check described in the previous paragraph but considering mobility across income quintiles. As with the case of deciles, the estimated coefficients on the lagged marginal tax rate are negative and significant (above 95%), but slightly higher: -0.79 and -0.53 (with standard errors of 0.23 and 0.24) for the specifications of pre-tax and post-tax income. Lagging the marginal rate further, results in estimated coefficients not statistically different from 0 (in the case of a two-period lag, the coefficients are positive and insignificant, but become negative -and still insignificant, for long horizons).

3.6 Additional Evidence on Taxation and Mobility

3.6.1 The heterogenous effects of taxes

This subsection analyses how different are the effects of changes in marginal taxation on income mobility across different levels of education. The degree of education can be a proxy for labour market skills. It is therefore interesting to analyse the income mobility dynamics for two sub-samples: households led by

[51] This is the only specification where the effect of $\tau_{i,t-2}$ is both positive and significant. Specifications when considering income quintiles (see Table3.14), post-transfer income (not reported) or further controls (not reported) do not find this coefficient to be significant.

Table 3.13: Robustness to different lags of the marginal tax rate (deciles of income)

	(1) move (D) pre-tax	(2) move (D) post-tax	(3) move (D) pre-tax	(4) move (D) post-tax	(5) move (D) pre-tax	(6) move (D) post-tax
τ_{t-1}	-0.569**	-0.414*				
	(0.24)	(0.24)				
τ_{t-2}			0.231	0.484*		
			(0.27)	(0.27)		
τ_{t-3}					-0.280	-0.264
					(0.30)	(0.30)
age (H)	-0.633**	-0.652**	-0.175	0.510	2.814	0.105
	(0.29)	(0.29)	(2.17)	(2.17)	(2.29)	(2.29)
age (W)	0.229	0.177	0.613*	0.271	0.096	0.255
	(0.28)	(0.28)	(0.34)	(0.34)	(0.40)	(0.40)
family size	-0.657	-0.804	-0.444	-0.435	-0.876	-0.963
	(0.61)	(0.61)	(0.65)	(0.65)	(0.69)	(0.69)
num children	0.128	-0.066	0.355	0.061	0.211	-0.132
	(0.62)	(0.62)	(0.67)	(0.67)	(0.71)	(0.71)
working wife	0.900	-0.082	0.435	0.039	0.781	0.961
	(0.84)	(0.84)	(0.89)	(0.89)	(0.96)	(0.96)
health status	1.236	0.613	2.123	2.292	1.619	2.295
	(1.27)	(1.29)	(1.40)	(1.43)	(1.52)	(1.54)
1^{st} stage F-stat	1583	1583	1395	1395	1180	1180
N	48507	48507	41118	41118	35449	35449

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile (D) or quintile (Q) of income. Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

Table 3.14: Robustness to different lags of the marginal tax rate (quintiles of income)

	(1) move (Q) pre-tax	(2) move (Q) post-tax	(3) move (Q) pre-tax	(4) move (Q) post-tax	(5) move (Q) pre-tax	(6) move (Q) post-tax
τ_{t-1}	-0.790***	-0.529**				
	(0.23)	(0.24)				
τ_{t-2}			0.129	0.153		
			(0.26)	(0.26)		
τ_{t-3}					-0.270	-0.321
					(0.28)	(0.28)
age (H)	-0.744***	-0.594**	1.484	1.687	2.203	0.395
	(0.29)	(0.29)	(2.03)	(2.02)	(2.18)	(2.16)
age (W)	0.574**	0.318	0.889***	0.682**	0.249	0.450
	(0.27)	(0.28)	(0.33)	(0.32)	(0.37)	(0.37)
family size	0.149	-0.720	0.607	-0.130	0.144	-0.103
	(0.59)	(0.58)	(0.62)	(0.62)	(0.66)	(0.66)
num children	-0.956	-0.173	-0.508	-0.491	-0.462	-0.738
	(0.60)	(0.59)	(0.63)	(0.63)	(0.67)	(0.67)
working wife	-0.297	-0.296	-1.639*	-1.022	-1.705*	-1.194
	(0.83)	(0.83)	(0.87)	(0.87)	(0.94)	(0.94)
health status	-1.328	-1.387	-0.668	-0.971	0.535	0.722
	(1.25)	(1.25)	(1.37)	(1.36)	(1.47)	(1.47)
1^{st} stage F-stat	1583	1583	1395	1395	1180	1180
N	48507	48507	41118	41118	35449	35449

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving to a different decile (D) or quintile (Q) of income. Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

a head that has completed college education and households whose head has a level of education below college graduate.

I now re-estimate Equation 3.3 with different dependent variables. First, I create a binary variable to describe upward movements in the income distribution, $up_{i,t}$, taking value 1 when the income of a households move up to a higher income rank. Similarly, I create a variable that considers downward movements in the income distribution, $down_{i,t}$ (with value of 1 when income rank moves to a lower position). For comparison I also define a variable determining immobility as $stay_{i,t} = 1 - mobility_{i,t}$.

Table 3.15 shows the estimated coefficients of the marginal tax rates in the income mobility variables described in the previous paragraph for the sample of non-college households.[52] It is worth noting that, for this sample of non-college graduates, the spouse's participation in the labour market is an important determinant of income mobility: a working spouse increases the likelihood of moving up by about 17% (columns 1 and 2, standard error of 1.09), while it reduces the probability of moving down by about 18% (columns 5 and 6, standard error of 1.44). A one percentage point increase in the marginal tax rate increases the probability of moving down to lower deciles of income by about 1% (columns 5 and 6, standard error of 0.4) and increases the likelihood of moving up in the income distribution to a higher extent, by around 1.5% (columns 1 and 2, standard errors of 0.27). Consistently with the results of Table 4.3, higher tax rates lead to a higher probability of remaining in the same income decile: point estimates of 0.915% and 0.728% (standard errors

[52]The estimated coefficients for the mobility variables are related by $\beta^{move} = \beta^{up} + \beta^{down}$, where $\beta^{move} = -\beta^{stay}$. In the regressions shown in Tables 3.15 and 3.15 we have that $\beta^{up} + \beta^{stay} + \beta^{down}$ is not usually 0. This is due to differences in the samples used: specifications for variable $up_{i,t}$ (columns 1 and 2) exclude households with an income in time $t-1$ belonging to the 10th decile, while specifications for variable $down_{i,t}$ (columns 5 and 6) exclude households with an income in time $t-1$ belonging to the 1st decile. This sample adjustment is done to account for the fact that households in the top (bottom) decile cannot experience further upward (downward) movements in the income distribution.

of 0.26) for specifications of pre-tax income (column 3) and post-tax income (column 4).[53]

Table 3.16 reports the results for a sub-sample of households led by a head with completed college education. The estimated coefficient on the dummy of working spouse is still large and significant, but smaller compared to those in Table 3.15.[54] Marginal tax rates are found to reduce the probability of moving down in the income distribution (columns 5 and 6), with point estimates of -1 and -1.3 (standard errors of 0.49) for the pre-tax and post-tax specifications, respectively. The effect on the probability of moving up in the income distribution, despite being positive, is not significant at usual confidence levels. For this sub-sample, higher tax rates also reduce mobility: by 1.1% when considering a distribution of post-tax income, although the point estimate of 0.5 is not significant for specifications of pre-tax income (standard errors of 0.51 in both cases).

To sum up, higher marginal tax rates increase mobility in both samples (less clearly in the case of college graduates). But the effects on upward and downward mobility are the opposite: non-college are, on average, more likely to move down in the income distribution, while college households are likely to move up (or, at least, less likely to move down) as a result of an increase in the marginal tax rates. These results, although should be taken with caution due to the increased uncertainty resulting from smaller samples, have important policy implications. Fiscal reforms that homogeneously reduce marginal tax rates seem to contribute to income mobility by making households with non-college education more likely to occupy relatively higher positions within the income distribution (and *vice versa* for college-graduated households).

[53] The results in the regressions in Tables 3.15 and 3.15 are robust to the inclusion of lag income as and additional control (not reported).

[54] This is probably the result of a higher percentage of employed wifes in the college-

Table 3.15: IV estimates: households without college educations

	(1) up pre-tax	(2) up post-tax	(3) stay pre-tax	(4) stay post-tax	(5) down pre-tax	(6) down post-tax
τ_t	-1.435***	-1.468***	0.915***	0.728***	0.916**	1.132***
	(0.27)	(0.27)	(0.26)	(0.26)	(0.39)	(0.39)
age (H)	-0.162	0.059	0.492	0.315	-0.164	-0.231
	(0.32)	(0.33)	(0.31)	(0.32)	(0.33)	(0.34)
age (W)	-0.300	-0.426	-0.114	-0.017	0.325	0.380
	(0.31)	(0.31)	(0.30)	(0.31)	(0.31)	(0.32)
family size	-0.824	-1.254*	0.187	0.369	0.302	0.665
	(0.69)	(0.70)	(0.67)	(0.68)	(0.77)	(0.78)
num children	0.992	1.616**	-0.225	-0.432	-0.498	-1.032
	(0.72)	(0.73)	(0.70)	(0.71)	(0.80)	(0.81)
working wife	16.792***	16.364***	-2.830**	-2.099*	-18.930***	-19.024***
	(1.09)	(1.09)	(1.10)	(1.10)	(1.44)	(1.44)
health status	-0.002	-0.523	-0.292	-0.942	1.082	2.748
	(1.47)	(1.48)	(1.39)	(1.39)	(1.66)	(1.68)
1^{st} stage F-stat	1181	1181	1181	1181	781	781
N	38417	38417	39766	39766	32229	32229

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving up to a higher income decile (columns 1 and 2), staying in the same decile (columns 3 and 4) or moving down to a lower decile (columns 5 and 6). Sample is restricted to observations where the head of the household has not completed college education. Households with income in time $t-1$ belonging to the 10th decile are excluded from specifications 1 and 2. Households with income in time $t-1$ belonging to the 1st decile are excluded from specifications 5 and 6. Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

Table 3.16: IV estimates: households with college education

	(1) up pre-tax	(2) up post-tax	(3) stay pre-tax	(4) stay post-tax	(5) down pre-tax	(6) down post-tax
τ_t	0.438	0.071	0.480	1.061**	-0.994**	-1.302***
	(0.54)	(0.56)	(0.51)	(0.51)	(0.49)	(0.49)
age (H)	-0.084	-0.654	0.352	0.503	0.190	0.320
	(0.77)	(0.77)	(0.55)	(0.52)	(0.48)	(0.50)
age (W)	-0.526	-0.029	0.308	0.029	0.029	0.007
	(0.74)	(0.74)	(0.52)	(0.49)	(0.46)	(0.48)
family size	1.377	0.793	0.038	0.340	-0.155	0.385
	(1.75)	(1.73)	(1.39)	(1.39)	(1.20)	(1.22)
num children	0.006	0.220	1.443	2.180	-1.849	-2.948**
	(1.72)	(1.71)	(1.36)	(1.36)	(1.17)	(1.19)
working wife	12.092***	13.099***	1.844	-0.323	-9.713***	-8.397***
	(2.11)	(2.15)	(2.01)	(2.03)	(1.86)	(1.88)
health status	-1.956	-1.830	1.129	0.395	-1.252	-0.691
	(3.43)	(3.46)	(3.02)	(3.01)	(2.84)	(2.74)
1^{st} stage F-stat	387	387	426	426	441	441
N	8259	8259	10556	10556	10316	10316

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving up to a higher income decile (columns 1 and 2), staying in the same decile (columns 3 and 4) or moving down to a lower decile (columns 5 and 6). Sample is restricted to observations where the head of the household has completed college education. Households with income in time $t-1$ belonging to the 10th decile are excluded from specifications 1 and 2. Households with income in time $t-1$ belonging to the 1st decile are excluded from specifications 5 and 6.Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

3.6.2 Do taxes increase mobility at the tails of the distribution?

This subsection analyses how changes in taxation affect mobility at the tails of the income distribution. Particularly, I estimate the effect of changes in the marginal tax rates on the probability that households in the bottom or top deciles of income remain in that position.

As in the previous subsection, I estimate Equation 3.3 with different dependent variable. For households in the bottom decile of income, I construct a new dependent (binary) variable, $up_{i,t}$, that takes value of 1 when the household moves up to a different decile. Similarly, variable $down_{i,t}$ takes value of 1 when a household in the top decile of income moves down in the distribution.[55]

Table 3.17 reports the estimates for households in the bottom decile (columns 1-3) and households in the top decile (4-5). The effect of an increase in the marginal tax rate on the probability that a poor households climbs to an upper position of the income distribution is negative and highly significant: point estimates range between -1.71 and -1.44 (standard errors of 0.23-0.26).

To understand the magnitude of this effect, consider a decrease of the marginal tax rate by 7 percentage points. This tax cut can explain around a quarter of the probability of leaving the bottom decile.[56]

The effect of tax rates on mobility in the top decile is less clear. The point estimates are negative, what would suggest that higher taxes increase the probability that households in the top decile remain in that position. However, the point estimates are associated to very high standard errors (in excess of 0.90) and therefore not significant at conventional levels.

---

graduated sample (75% *versus* 68%).

[55] The aggregate probability of moving away from the tails of the income distribution, i.e. $1 - P^{1,1}$ and $1 - P^{10,10}$ of mobility matrix P in Equation 4.3 are shown in Figure A5.

[56] The average likelihood of leaving the bottom decile is 45%.

Table 3.17: IV estimates: households in bottom and top deciles

	(1) up pre-tax	(2) up post-tax	(3) up post-trans	(4) down pre-tax	(5) down post-tax	(6) down post-trans
τ_t	-1.564***	-1.719***	-1.441***	-0.620	-0.713	-0.644
	(0.23)	(0.25)	(0.26)	(0.94)	(0.91)	(0.91)
age (H)	1.237	1.218	2.850	0.303	0.764	2.790
	(0.81)	(0.77)	(4.51)	(0.63)	(0.66)	(5.09)
age (W)	-0.303	-0.269	0.004	-0.103	-0.364	0.133
	(0.80)	(0.76)	(0.78)	(0.63)	(0.64)	(0.60)
family size	0.238	-0.831	-0.201	-2.026	-0.997	-0.058
	(1.54)	(1.55)	(1.77)	(1.52)	(1.61)	(1.63)
num children	2.588	4.343***	4.509**	-0.742	-2.839*	-4.887***
	(1.60)	(1.65)	(1.95)	(1.62)	(1.69)	(1.78)
working wife	22.714***	22.091***	20.406***	-16.340***	-17.331***	-17.583***
	(1.90)	(1.92)	(2.02)	(3.78)	(3.65)	(3.96)
health status	-1.449	-0.469	1.695	8.676**	10.286**	7.548*
	(2.89)	(3.01)	(3.26)	(3.91)	(4.46)	(4.58)
1^{st} stage F-stat	456	439	438	90	91	73
N	6961	6830	6313	3330	3379	3170

Note: IV (2SLS) estimates of the effects of marginal tax rates on the probability of moving up to a higher income decile for households in the bottom decile (columns 1-3) or the probability of moving to a lower decile for households in the top decile (columns 4-6). Tax reforms are used as an instrument for the marginal tax rate τ_t. Specifications also differ on how income is measured (pre-tax AGI, post-tax AGI and post-tax and post-transfers AGI). Time span is 1967-1996. All regressions use panel data and include time and individual fixed effects. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

3.7 Conclusion

Rising inequality has triggered a debate on what is the role that fiscal policy should play in addressing economic disparities. However, taxes are likely to have an impact on other features of the income distribution beyond inequality. This chapter considers the effect of fiscal policy on income mobility. I exploit the variation in marginal tax rates originated by several reforms in the US to estimate how likely is that the relative position of a household in the income distribution changes when taxes vary. The resulting evidence suggest that lower marginal tax rates foster mobility along the income distribution. Particularly, an increase of one percentage point in the marginal tax rate causes a decline of around 0.8% in the probability that a household's income changes to a different decile of the income distribution. A change in the marginal tax rate of 7 percentage points accounts for around a tenth of the average likelihood of movements in the income distribution (and around a quarter of the average probability that a household in the bottom decile moves to a higher position). The mechanism that brings about this effect seems to be based on the distortions induced by taxes in the labour market decisions.

These empirical results have important implications for the design of fiscal policy. Tax reforms that reduce marginal rates are more likely to increase equality of opportunity (as measured by the degree of income mobility). This is because an attenuation of the distortionary effects of taxes in the labour market would make households more likely to take advantage of economic opportunities and move up in the income distribution. Therefore, fiscal policies that aim to reduce inequality should weight the trade-off in households' welfare induced by the effect on income mobility.

This analysis can be extended in several ways. First, this chapter highlights the need to address the potentially different effects of taxation on income inequality and income mobility. A comprehensive analysis of fiscal policy should jointly address these issues. The interaction of a progressive tax schedule with income inequality and mobility necessitates a structural general equilibrium model that generates heterogeneity in the spirit of Aiyagari (1994) while allowing for relevant labour supply decisions. This framework would allow for a quantification of the impact of welfare of fiscal policies that address inequality, both in terms of income and wealth.

This chapter has restricted its attention to mobility in the short run. Another interesting avenue is to explore the effects of taxation on social (or intergenerational) mobility. A low degree of association between parents' and children's income is an indicator of higher equality of opportunity. Nybom and Stuhler (2014) note the importance of shocks affecting the parents in determining current intergenerational mobility. It is therefore highly relevant from a policy standpoint to understand whether major fiscal reforms as Tax Revenue Act of 1986 can have a noticeable impact on children's future position in the income distribution.

Finally, my analysis can also be extended to understand the effects of taxation on other dimensions such as job mobility and the decision of becoming an entrepreneur. The macroeconomic literature that investigates the sources of wealth inequality has often relied on entrepreneurship as a key element to understand why rich households accumulate so much wealth (see De Nardi (2015)).[57] Whether the incidence of personal income or corporate income taxation is a factor determining the decision to become self-employed (beyond wealth accumulation) has important policy implications.

[57]However, Hurst and Lusardi (2004) highlights that wealth accumulation is only an important factor on the entrepreneurial decisions for those individuals above the 95% percentile of wealth.

Chapter 4

Do Tax Changes Affect Intergenerational Mobility?

4.1 Introduction

The transmission of income status across generations is a central issue in social sciences. This is a particularly relevant subject for welfare economics, since a lack of mobility across generations implies the absence of equality of opportunity. This topic has recently attracted much attention in the public debate and in policy circles.[1] But what can policymakers do to address this phenomenon?

This chapter explores how fiscal policy may contribute to the transmission of the income status from parents to children. This is an important question from a policy standpoint, since it explores a new dimension through which fiscal policy can affect the economy and the society. Recently, the rising income inequality in the US has started a debate on how fiscal policy should be conducted to address this phenomenon (see Piketty and Saez (2007)). However, little is known on how changes in taxation facilitates social mobility.

[1] See Krueger (2012).

In this chapter, I employ data from the Panel Study on Income Dynamics (PSID) and matches pairs of fathers and sons between 1967 and 2012. I first compute the degree of intergenerational income mobility and its recent trends in the US. To do this, I estimate the most frequent parameter in this literature: the intergenerational elasticity (IGE) of income, which quantifies how much of the income differences are passed from fathers to sons. Next, I use the TAXSIM simulator to construct a measure of changes in federal tax liabilities that are the result of legislated changes in the tax code. This allows me to evaluate how fiscal policies affect actual tax liabilities in the fathers generation. I then estimate how those changes in taxation interact with the elasticity of income mobility between two adjacent periods.

I find that changes in the federal income tax code do affect intergenerational mobility Particularly I find that the difference in the elasticity between a family that faces a 1,000-dollar change and a family who does not is around 5 percentage points. A potential mechanism that may bring about these results is one based on the decision of parents to invest in the stock of human capital of their children. Reforms that reduce the tax liabilities that parents face, enable them to use these extra resources in funding education and providing their children with better opportunities, what would translate in higher intergenerational mobility. Overall, the results of this paper suggest that, through income taxation, fiscal policy can impact on the equality of opportunity between generations. This is an important dimension that policy actions should take into account when considering their medium run effects.

This chapter relates to an extensive literature on intergenerational mobility (or social mobility) as surveyed in Fields and Ok (1999), Black and Devereux (2011) and Jäntti and Jenkins (2015).[2] This literature seeks to quantify the de-

[2]Fields and Ok (1999) and Jäntti and Jenkins (2015) also survey the literature on *intra-*

gree of association between parents and children income, usually by computing the elasticity of income in both generations.[3]

The present chapter follows many elements of the empirical analysis described in Solon (1992), Zimmerman (1992). These classic papers highlight potential econometric issues in estimating the degree of intergenerational mobility, finding that previous estimations of the IGE suffered from downward bias due to transitory shocks that affect income in both generations. Haider and Solon (2006) and Grawe (2006) identify other sources of biases in the analysis of intergenerational mobility due to life cycle considerations.[4]

An important strand of the literature has explored the determinants of intergenerational mobility. Piketty (2000) surveys theories that explain the persistence of income across generations, mainly due to the transmission of productive abilities. Intelligence and innate abilities are also potential determinants of upward social mobility. Hassler and Mora (2000) develop a model where endogenous growth increases the return of individuals born with higher cognitive skills, and thus resulting in higher intergenerational mobility. In this model, when growth is low, the return of innate cognitive abilities is lower compared to other social assets. This results in lower intergenerational mobility, since the children of the people with high cognitive abilities have an ex-ante advantage and are more likely to inherit the income status of their parents. Aghion et al. (2015) explores the possibility that intergenerational mobility is caused by the degree of innovation and entrepreneurship of the individuals. The authors employs cross-state panel data from the US to find a positive correlation between innovativeness (measured by the number of patents granted)

generational mobility: the movements across income ranks of people in the same generation between two periods of time.

[3]This object is often referred to as the intergenerational elasticity of income (IGE), see Fields and Ok (1999).

[4]See Section 4.2 for a more detailed description of these econometric issues.

and upward intergenerational mobility. The study finds that this correlation is explained by the entrance of new innovators. Other potential determinants of social mobility are related with geographical factors.[5] Chetty and Hendren (2015), investigate how neighbourhoods affect intergenerational mobility through childhood exposure effects. The authors find that the children of families that move from areas with low mobility to areas with higher mobility are more likely to rise in the income distribution. Using quasi-experimental variation they find that the factors explaining this increase in social mobility are related to the features of the new neighbourhood, such as higher-quality schools. This evidence is consistent with Chetty et al. (2016), that find that a randomised public program that helped to relocate families to better neighbourhoods (the Moving to Opportunity experiment) resulted in higher future earnings for their children.

This chapter relates to the above literature on the determinants of intergenerational mobility by analysing the role of fiscal policy in reducing income differences across generations. The idea that aggregate shocks affecting the parents generation can impact on intergenerational mobility has already been explored in the literature. Nybom and Stuhler (2014) study how events that affect the distribution of income of the parents may have important effects on the following generation. The authors construct a model that shows how changes in policies and institutions can impact on intergenerational mobility across multiple generations. They use a structural reform that raised the compulsory schooling in Sweden to test the implications of the model.

To the extent that intergenerational mobility and equality of opportunity are closely related, the literature has also explored how the transmission of

[5]Chetty et al. (2014a) explicitly quantifies the geographical differences in intergenerational mobility in the US using a large administrative dataset. The authors find that there is considerable variation in mobility across commuting zones (aggregation of counties).

income status has evolved over time. Chetty et al. (2014b) use a large US administrative dataset to measure the evolution of rank-based indicators (the correlation of parent and child income ranks and the probability that a child reaches the top of the income distribution). They find that mobility has remained fairly stable for the cohorts born between 1973 and 1993. Aaronson and Mazumder (2008) construct a new dataset matching individuals from the US Census to synthetic families in the previous generations (mainly determined by the state of birth) and find that mobility, measured by the IGE, has declined since the 1980s. This contrasts with evidence from Lee and Solon (2009), that use data from the PSID and conclude that the cohorts born between 1952 and 1975 do not exhibit major changes in intergenerational mobility, when measured by the elasticity of parent-child income. This chapter extend those results with newer data from the PSID, computing both the frequently used IGE parameter and rank-based measures; and finding that mobility has not suffered major fluctuations in the last two decades.

The literature or intergenerational mobility has almost exclusively focused on income dynamics rather than wealth. This contrasts with the attention that wealth and its distribution have attracted in the last years (see for example Piketty (2014), Saez and Zucman (2014) and Piketty and Zucman (2015)). However, a literature investigating the transmission of wealth across generations has started to develop. Benhabib et al. (2015) analyse what factors are needed in a heterogeneous-agents model to match the distribution of wealth and the degree of social mobility across generations. The authors conclude that the skewness and persistence of earnings, differential saving and bequest rates and the existence of capital income risk in entrepreneurial activities are all necessary factors to account for this matching between the model and the data. Recently, Adermon et al. (2015) employ Swedish data to find that the rank correlation between parents and children wealth ranges from 0.3 to 0.4

Lastly, this chapter also relates to the literature that investigates the effects of fiscal policy using legislated tax reforms such as Romer and Romer (2010) and Barro and Redlick (2011). Romer and Romer (2010) produce a narrative series of federal tax changes in the US, finding substantial aggregate effects of on economic activity. The current chapter quantifies the size of these reforms for each family in the PSID using the TAXSIM simulator, in a similar vein to Gruber and Saez (2002) and Alloza (2016).

This chapter is structured as follows. Section 4.2 describes issues that arise when estimating the IGE and explores the recent trends in intergenerational mobility in the last years. Section 4.3 explains the empirical strategy that is used to estimate the effects of tax variations on the intergenerations elasticity. The results of these estimations are presented in Section 4.4. Section 4.5 tests the robustness of the results to different specifications. Lastly, Section 4.6 concludes and offers some directions for future research.

4.2 Intergenerational Mobility in the US

This section explores the degree of intergeneration mobility in the US. I employ data from the PSID between 1967 to 2012 to match the income of fathers and their sons.[6] I restrict the analysis to pairs of fathers and sons of a certain age and with positive labour income.

To quantify the degree of association of income between two adjacent generations, I estimate the following regression:

$$\log\left(income_i^{son}\right) = \alpha + \beta \log\left(income_i^{father}\right) + \varepsilon_i \tag{4.1}$$

[6]Section 4.5 includes both sons and daughters in the analyses.

where $income_i^{son}$ is the before-tax labour income of the individual (son) i, and $income^{father}$ is the before-tax labour income of his father.[7] The coefficient β is often referred to as the intergenerational elasticity (henceforth IGE) of income (see Solon (1992) or Jäntti and Jenkins (2015)). This object measures the degree of transmission of income differences from one generation to another. For example, if we consider the case of a father whose earnings are twice the average given a value of the IGE $\beta = 0.5$, that would imply that the son would be expected to have a level of income 50% higher than the average. For a value of $\beta = 0.1$, the 100% income difference in the father's generation, would only add to a 10% difference in the son's generation.

An estimation of Equation 4.1 implies however two relevant empirical issues. On the first hand, Atkinson (1980) notes how transitory errors that affect the previous generation income may result in downwardly inconsistent estimations of β in Equation 4.1. A solution to this problem of attenuation bias has been explored in Solon (1992), Solon (1989) and Zimmerman (1992). Solon (1992) proposes to consider multi-year averages of parental income instead of a single year and shows this could lead to higher estimates of the IGE parameter.

There is, however, a second econometric issue. The solution to the problem of attenuation bias mentioned above, rests on the assumption that transitory errors behave as classical errors. However, this may not be the case if these error-in-variables are age-dependent. Following Haider and Solon (2006), consider a relationship between permanent income y_i and actual yearly income $y_{i,t}$:

$$y_{i,t} = \lambda_t + \nu_{i,t} \tag{4.2}$$

[7]Note that this chapter does not aim at establishing comparisons of living standards between generations, but rather at exploring the likelihood that sons can generate a level of income that provides them with an income status similar to that of their parents.

where $\nu_{i,t}$ is a transitory error and λ_t is an age-dependent parameter that controls how permanent income translates into current income during different periods of the life cycle of a given generation.

When $\lambda_t = 1$ for all periods t, and conditional on certain properties of $\nu_{i,t}$ (see Haider and Solon (2006)), the errors in actual income are transitory and the classical error model is a correct representation of the annual income process. In that case, the only remaining econometric issue is the attenuation bias mentioned above. However, it could also be the case of $\lambda_t < 1$ for workers starting their career, and $\lambda_t > 1$ later in their life cycle. In this situation, a time-dependent λ_t induces another source of inconsistency in the estimation of the IGE parameter β in Equation 4.1, known as the life cycle bias (see Haider and Solon (2006)).[8]

The consequence of this problem is that the estimate of β (the IGE) suffers from a downward bias earlier in the life cycle and is overestimated at a later stage. The solution to this problem is to estimate the degree of intergenerational mobility when the λ of each generation is approximately similar, so we would be effectively comparing individuals at a similar stage of their life cycle. Grawe (2006) computes the degree of attenuation and life cycle bias present in the US and finds that using observations of income for fathers and sons at similar points in their life cycle (or points in time when $\lambda = 1$ and therefore measurement errors are approximately classical) reduces the life cycle bias.[9]

[8]The models that dispute the assumption of transitory income shocks having the same properties as classical measures and, therefore, assuming an income model similar to Equation 4.2 are known as generalized-error-in variables. See Haider and Solon (2006) and Jäntti and Jenkins (2015).

[9]In a recent paper, Nybom and Stuhler (2016) use nearly actual lifetime income profiles of fathers and sons in Sweden to show that the generalized-error-in-variables model proposed by Haider and Solon (2006) and Grawe (2006) do not fully eliminate the life cycle bias. Moreover, the income model described in Equation 4.2 rests on the hypothesis that the parameter λ is the same for both generations. This assumes that the relationship between permanent and actual income has remained unchanged in the two or three decades that separate the fathers and sons generations, ignoring potential delays in the labour market

To account for the econometric issues above, in the next subsections I (i) explore the trends of intergeneration mobility using similar moments in the life cycle of fathers and sons (to reduce the life cycle bias) and (ii) estimate the β in Equation 4.1 using multi-year averages of income to reduce the attenuation bias.

4.2.1 Recent Trends in Intergenerational Mobility

To gain some insights on the recent evolution of the degree of transmission of the income status between fathers and sons, I estimate Equation 4.1 for each year between 1992 and 2012. I establish a comparison of the annual (pre-tax) income of fathers and sons when both generations are separated by time periods of 25 years (i.e. I am effectively using data from 1967 to 2012). I consider only the subset of the PSID sample that is considered representative at the national level (the so-called core sample).[10] Further to this, I restrict the sample to fathers and sons who are head of households, earn a positive labour income, and are aged 30 to 40. This last criterion aims at reducing the life cycle bias mentioned at the beginning of this section, while keeping the number of observations big enough to obtain precise estimates.

Figure 4.1 shows the evolution of the IGE parameter between 1992 and 2012. The elasticity fluctuates around an average value of 0.30. Higher values of the parameter indicate a greater degree of immobility (more intergenerational persistence of the income status from fathers to sons). The elasticity seems to pick up slightly in the 1990s and fall moderately in the following decade, only to pick up again towards the end of the sample. Despite these

entry due to more extended periods of education.

[10]The PSID survey was originally created by merging two samples: the Survey Research Center (SRC) or core sample (representative at the national level), and the Survey of Economic opportunity (SEO) or Census sample, which over-represents low income households. The results of this section are robust to including only the SRC core sample or both of them.

slight fluctuations, the evolution of the intergenerational elasticity does not show significant variations. This result is in line with Lee and Solon (2009), which also compute the IGE parameter and Chetty et al. (2014b), which measure intergenerational mobility with rank correlations of parental and children's income employing a large administrative database.

When estimating Equation 4.1 with a pooled sample of all years, the value of the estimated IGE is 0.30 (with a standard error of 0.3). This value indicates that during the sample period considered, the income difference of a father with respect to other fathers at that time, is transmitted to the next generation in around 30% (on average).

To further analyse the recent trends of intergenerational mobility in the US, I compare the annual evolution between 1992 and 2012 of the IGE and the Gini coefficient, a commonly used measure of inequality.[11]

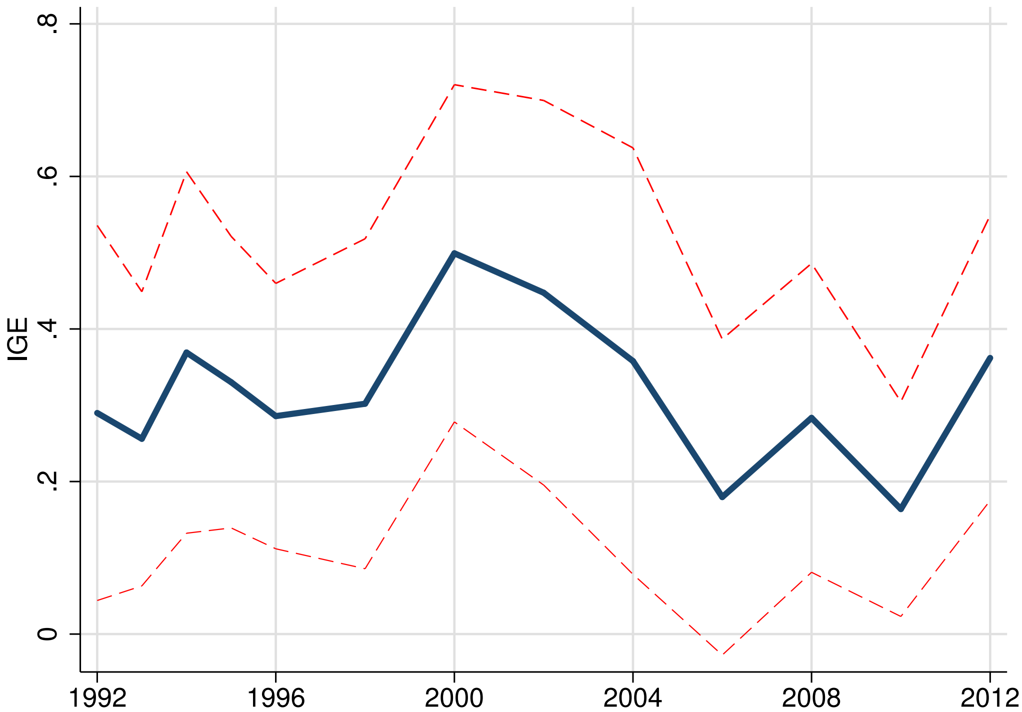
Figure 4.2 plots such a relationship between mobility and inequality. The two show a positive co-movement, with a significant correlation coefficient of 0.53. This figure seems to indicate that times of higher intergenerational mobility are associated with times of greater inequality.

This concept has recently drawn attention from policy makers (Krueger (2012)), since relates two concepts that are of great interest for a society: equality of opportunity across generations and inequality. When the relationship between these phenomena is explored using a cross section of countries, Figure 4.2 is often referred to as "The Great Gatsby" curve.[12] The importance of this relationship has also been recently highlighted by Chetty et al. (2014a), which find that US areas that exhibit high levels of inequality also tend to

[11]The Gini coefficient ranges from 0 (all income is equally earned across the entire population) and 1 (the point of maximal inequality, where only one person earns the totality of the income).

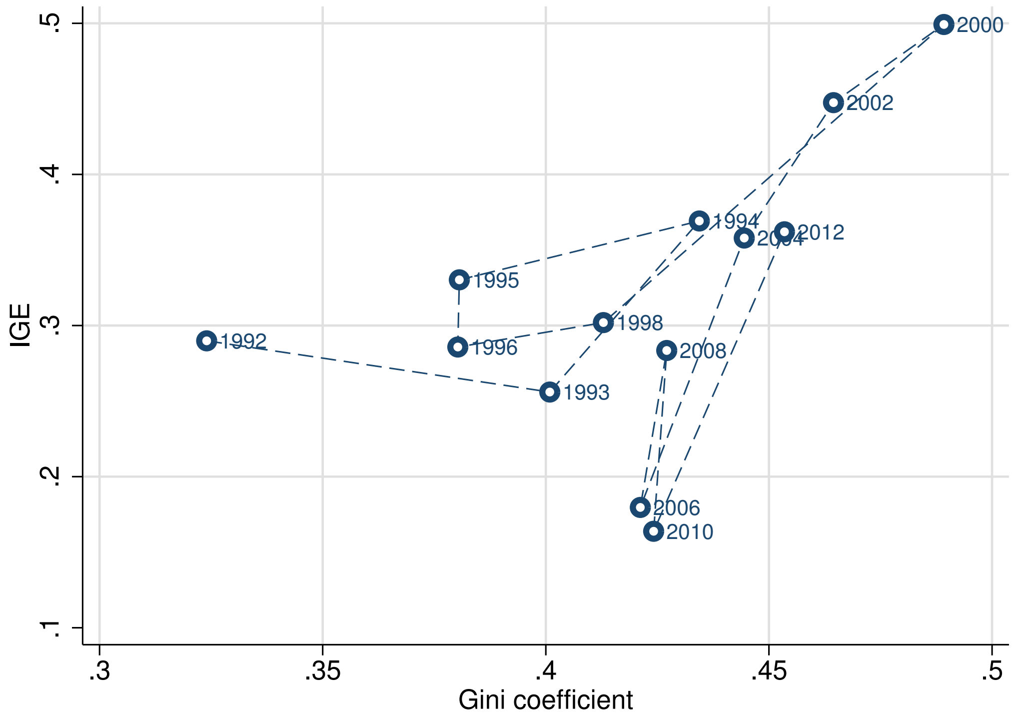
[12]The Great Gatsby curve owes its name to the then-chairman of the U.S. Council of Economic Advisors (see Krueger (2012)).

Figure 4.1: Evolution of the IGE



Note: The blue solid line shows the evolution of the intergenerational elasticity of income between 1992-2012 (using data since 1967), defined as the β in Equation 4.1. The sample is restricted to fathers and sons aged 30-40 with a time gap between generations of 25 years. Only individuals that are heads of households and earn a positive labor income are considered. The sample is further restricted to members of the PSID considered to be representative at the national level (core sample). The red dashed lines represent two (robust) standard errors.

Figure 4.2: The Great Gatsby Curve



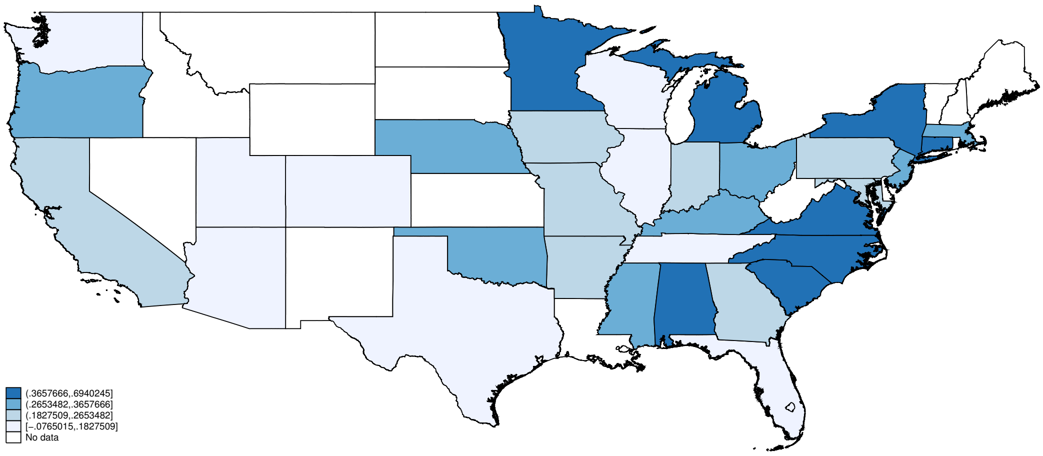
Note: The horizontal axis shows the degree of intergenerational mobility (higher values of IGE mean more immobility). The vertical axis shows the Gini coefficient (a measure of the degree of income inequality). The sample period refers to 1992-2012 (using data since 1967). The sample is restricted to fathers and sons aged 30-40 with a time gap between generations of 25 years. Only individuals that are heads of households and earn a positive labor income are considered. The sample is further restricted to members of the PSID considered to be representative at the national level (core sample). The red dashed lines represent two (robust) standard errors.

display less upward mobility for children from low-income families.

Lastly, Figure 4.3 shows a map of the IGE across US states. The results from this exercise show substantial geographical variation.[13] The Great Plains and the West Coast show a higher degree of mobility compared to the East Coast. Interestingly, Chetty et al. (2014a) uses a large administrative dataset and find that both the Great Plains and the West Coast exhibit a high level

[13]States that do not have enough number of observations are left out of the sample. The geographical patterns remain qualitatively similar when these states are included in the analysis.

Figure 4.3: IGE across US States



Note: Intergenerational elasticity of income across US states between 1992-2012 (using data since 1967), defined as the β in Equation 4.1. The sample is restricted to fathers and sons aged 30-50 with a time gap between generations of 25 years. Only individuals that are heads of households and earn a positive labor income are considered. The sample is further restricted to members of the PSID considered to be representative at the national level (core sample). States with insufficient data (less than 40 observations) are dropped from the sample.

of upward mobility (measured as the rank correlation between parental and children income), while the Southeast remains the area with the lowest degree of upward mobility.

4.2.2 Measuring Intergeneration Mobility Using Permanent Income

As mentioned at the beginning of this section, the use of annual income data may result in downwardly biased estimations as a consequence of measurement error (attenuation bias). Equation 4.2 decomposes actual income into permanent and a transitory components. In this section, we follow, among others, Solon (1992) and estimate the IGE using permanent income. To do this I take multiyear averages of reported income. This method aims at reducing the attenuation effect that arises from the presence of transitory shocks in

Equation 4.2.

By looking at narrowly defined age groups, I estimate the IGE of two adjacent generations at similar moments in their life cycle. Particularly I look at average earnings from fathers and sons when they are 40 to 50 years old (in an attempt to measure income at the peak of their careers), and when they are 30 to 40 years old.

Measuring income in this way reduces the number of observations (since each person now appears only once during the sample period), but takes into account both econometric issues related to attenuation and life-cycle biases mentioned at the beginning of the section.[14]

Table 4.1 shows the results of estimating β in Equation 4.1 when income is a multi-year average. When considering income at the peak of their life-life cycle (40 to 50 years old), the IGE is estimated to be around 0.38 (standard error of 0.06), as reported in the first column of Table 4.1. As noted in Solon (1992), this number is higher than estimations using actual instad of multiyear averages of income (as in Figure 4.1). An estimation of the intergenerational elasticity of around 0.38 implies that for a father who earns 50,000 dollars less than the average, around 38% of that difference will be passed on to his son (i.e. the son will earn around 19,000 dollars less than the average). This estimate is slightly smaller when we consider as measure of income the average earnings from years 30 to 40. In this case (column 3 in Table 4.1), the IGE is 0.33 (standard error of .04). Both estimates are similar when we include a set of demographic and socioeconomic variables as controls, with an intergenerational elasticity estimate at around 0.30-0.31 (with standard errors

[14]To increase the degrees of freedom, the sample incorporates both the SRC core and SEO subsamples from the PSID. Probability weights are used to render the resulting sample representative of the US population. However, results are robust to the inclusion or omission of the SEO subsample.

Table 4.1: IGE using Average Income

	(1) age 40-50	(2) age 40-50	(3) age 30-40	(4) age 30-40
$income^{father}$	0.378***	0.309***	0.326***	0.301***
	(0.06)	(0.07)	(0.05)	(0.04)
controls	NO	YES	NO	YES
N	1201	1201	1793	1793

Note: Estimations of β in $\log\left(income_i^{son}\right) = \alpha + \beta \log\left(income_i^{father}\right) + \varepsilon_i$. Columns (1) and (2) compute income as the multiyear-average during ages 40 to 50. Columns (1) and (2) compute income as the multiyear-average during ages 30 to 40. Columns (2) and (4) include as controls the ages of the father and son, the number of children in the son's family, and dummy variables if the son is married, if the son's wife is engaged in the labour market and if the son's parents come from a poor economic background. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

of 0.04 to 0.07).[15]

Overall these numbers suggest that, on average, around a third of the income differences exhibited by fathers are passed on to their sons in the US during this sample period.

4.2.3 Transitions Across Income Categories

In this subsection I analyse intergenerational mobility with a different framework as the one summarised by Equation 4.1. Following Chetty et al. (2014b), the joint distribution of income of fathers and sons can be decomposed in two elements: the joint distribution of father and son income (which is also known as the copula of the distribution) and the marginal distribution of father and son income. While the IGE parameter in Equation 4.1 picks up features from both the copula and the marginal distribution, I will now consider quintile transition matrices, which solely depend on the copula.

The importance of using transition matrices to analyse intergenerational

[15]Control variables include the ages of the father and son, the number of children in the son's family, and dummy variables if the son is married, if the son's wife is engaged in the labour market and if the son's parents come from a poor economic background.

mobility is twofold.[16] On the one hand it fully controls for marginal distributions, allowing to focus on the joint distribution of income as mentioned above. And on the other hand, it allows us to analyse asymmetries in the transmission of income status. This is because the β coefficient in Equation 4.1 is only informative of the *average* persistence of income (i.e. it is not informative of how big the fluctuations around the average are). The transition matrices, however, allow for these features by indicating whether, for example, mobility is higher at the top or at the bottom of the distribution.[17]

To use transition matrices, I consider an ordering of income for generation t in 5 different ranks (quintiles of income). The intergenerational mobility process can be represented by the following equation:

$$s_t = P s_{t-1} \tag{4.3}$$

The vector s_{t-1} summarises the probability distribution of fathers income in period $t-1$, while the vector s_t refers to the sons income. Notice that since s_t and s_{t-1} are uniform distributions by construction, the matrix P fully characterises the mobility process by determining the probability that a son with a father belonging to income group n remains in the same quintile as his father (entry $P^{n,n}$ in matrix P) or transits to another quintile $k \neq n$.

Table 4.2 computes the matrix of probability of transitions for our 1967-2012 sample of fathers and sons, when income is measured as the multiyear average between ages 40 and 50. The main diagonal shows the probability that a son achieves the same income quintile as his fathers. Elements above (below)

[16] Jäntti and Jenkins (2015) recognise the importance of using transition matrices to analyse intergenerational mobility and highlight the fact that the literature has neglected the use of this object.

[17] An alternative would be to consider nonparametric estimates. Although the literature has not profusely explored this avenue, some studies (see for example Eide and Showalter (1999)) have estimated quantile regressions of Equation 4.1.

the diagonal show upward (downward) movements in the income distribution between both generations.

The sum of the diagonal elements of the matrix (the trace) summarises the degree of intergenerational immobility present in the society. The extreme case of perfect immobility (where the income status of the fathers predetermine that of the sons) would imply that the matrix P is equal to the identity matrix. The case of total origin independence (no relation between fathers' and sons' income ranks) would mean that matrix the P has the same value in each entry of the matrix (in our case, that would be 4%). The matrix in Table 4.2 shows a moderate level of persistence of income status. The Shorrocks Index (see Shorrocks (1978b)), which measures the degree of immobility, has a value of 0.83 (with 0 being a completely immobile society).

Table 4.2 shows some other interesting patterns at the extreme of the distributions. A third of the poorest sons also have the poorest fathers. This is even more dramatic when considering the top quintile: around 42% of the richest parents have their sons occupying the same income position one generation later. Focusing on the most extreme intergenerational movements, around 8% of the sons born to the poorest fathers, make it to the top of the income distribution. Conversely, 7.4% of the sons of the richest fathers fall to the bottom quintile of income. Interestingly, Table 4.2 also shows a certain degree of asymmetry, which would imply that intergenerational mobility may differ along the income distribution.

4.3 Empirical Strategy

In this section I present the empirical strategy that I use to understand how changes in fiscal policy may affect the transmission of economic status across

Table 4.2: Matrix of Transition of Probabilities

	Destination				
Origin	Q1	Q2	Q3	Q4	Q5
Q1	33.33	26.67	18.33	13.33	8.33
Q2	22.68	27.84	22.68	19.59	7.22
Q3	12.64	24.73	21.98	24.18	16.48
Q4	14.13	11.96	20.11	27.72	26.09
Q5	7.39	10.23	19.32	21.02	42.05

Note: Probabilities that a son moves to a quintile of income with respect to the quintile of income of his father. Rows indicate quintiles of income of fathers and columns refer to quintile of incomes of sons. Income is measured as the multiyear average between the ages of 40 and 50.

generations. Particularly, I estimate the following regression:

$$\begin{aligned} \log\left(income^{son}_{i,t}\right) &= \alpha + \beta \log\left(income^{father}_{i,t}\right) + \gamma \Delta TAX_{i,t} \\ &\quad + \delta \log\left(income^{father}_{i,t}\right) \Delta TAX_{i,t} + \xi_{i,t} \end{aligned} \tag{4.4}$$

where $income^{son}_{i}$ is the before-tax annual labour income of the individual (son) i, and $income^{father}$ is the before-tax annual labour income of his father. Both fathers and sons are aged between 30 and 40 years, and each generation is separated between 25 years.

$\Delta TAX_{i,t}$ is defined as:

$$\Delta TAX_{i,t} = TAX^{t}_{i,t} - TAX^{t-1}_{i,t} \tag{4.5}$$

where $TAX^{t}_{i,t}$ is the actual tax liability (in thousands of dollars) that the father i faces in year t given his income and the tax code that was present in that year. $TAX^{t-1}_{i,t}$ is the counterfactual tax rate that the father i would have paid with his current income had the tax code in $t-1$ still be present in time t. That is, $TAX^{t}_{i,t}$ effectively computes the variation in the tax bill that a father faces as a consequence of a change in the tax code. In this way, positive values of $TAX^{t}_{i,t}$ indicate that the father has seen his tax liabilities increased as a consequence of a hike in the features of the tax code that are relevant according to his specific circumstances.

To compute $TAX_{i,t}^{t}$ and $TAX_{i,t}^{t-1}$ I employ TAXSIM, a tax simulator hosted by the NBER. When computing the tax liabilities I take into account tax-relevant features of both the father and his wife (if he is legally married). Particularly, I take into account the tax year code, marital status (I assume that married people jointly file taxes), number of dependants (including those under 17 years), labour income from the head of the household and his spouse, asset income (arising from rentals, dividends or interests), taxable pensions, Social Security Income, property taxes and deductions on mortgage interests.[18]

Equation 4.4 departs from Equation 4.1 by adding two additional terms related to changes in fiscal policy. The interaction of the $TAX_{i,t}^{t}$ and father's income is of particular interest, because it summarises how changes in tax liabilities affect the degree of transmission of income differences between fathers and sons.

4.4 Results

This section describes the results of the estimation of Equation 4.4, and Table 4.3 summarises the findings.

The first column of the table estimates Equation 4.4 setting $\beta = \delta = 0$, i.e. a version of Equation 4.1.[19] The results indicate an IGE of almost 0.35 (standard error of 0.02), what implies that on average, slightly more than a third of the income differences in the fathers' generation are transmitted to

[18]Due to data availability, this analysis focuses on federal income tax liabilities only (state and payroll taxes are not contemplated).

[19]The only difference with the sample used in Figure 4.1 is the inclusion of the SEO subsample and the use of probability weights (to render the sample representative). Results are robust to the omission of this subsample, as shown in Section 4.5.

the sons' generation.

Columns 2 and 3 of Table 4.3 include the variable $TAX_{i,t}^{t}$ additively and interacting with fathers income, respectively. The inclusion of these variables does not significantly alter the IGE parameter. When only $TAX_{i,t}^{t}$ is included, the point estimate becomes -0.005 (standard error of 0.02). This implies that an increase of 1000 dollars in taxes for the father, is associated with a decrease of around 0.5% of the son's income in the next generation.

Adding an interaction term (column 3) brings about an important result: the effect of the father's income on his son's income (i.e. the transmission of income status) depends on tax policies that affect the father. Or in other words, the IGE varies with respect to taxation.

To see this, consider the intragenerational elasticity of a pair of father and son where the father has not been affected by a tax policy. According to the coefficient in the regression represented in column 3, the IGE is 0.32. Consider now a father-son pair where the father faces a 1,000-dollar increase in tax liabilities. This results in an elasticity of 0.37.

Column 4 of Table 4.3 includes year dummies in the regression of Equation 4.4 that aim to account for potential macroeconomic shocks that affect in the same fashion to all individuals. The results are not substantially altered, and the intergenerational elasticity evaluated at the average value of $TAX_{i,t}^{t}$ (0.28) is 0.314 (compared to a value of 0.304 when the time dummies are omitted).

Column 5 adds some demographic and socioeconomic controls (as mentioned in Section 4.2 and the footnote of Table 4.3). The results are in line with previous specifications: the difference between the IGE of a family whose father is affected by a tax reform of 1,000 dollars and a family who is not amounts to 5.8 percentage points.

Table 4.3: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
$income^{father}$	0.348***	0.346***	0.321***	0.288***	0.288***	0.078**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
ΔTAX		-0.005	-0.687***	-0.954***	-0.729**	-0.540***
		(0.02)	(0.20)	(0.29)	(0.28)	(0.14)
$income^{father}$			0.055***	0.076***	0.058**	0.042***
$\times\Delta TAX$			(0.02)	(0.02)	(0.02)	(0.01)
time dummies	NO	NO	NO	YES	YES	NO
fixed effects	NO	NO	NO	NO	NO	YES
controls	NO	NO	NO	NO	YES	NO
N	4130	4130	4130	4130	4130	4130

Note: OLS estimations of Equation 4.4. Sample period from 1967 to 2012. The sample is restricted to fathers and sons aged 30-40 with a time gap between generations of 25 years. Column 5 includes the following controls: ages of the father and son, the number of children in the son's family, and dummy variables if the son is married, if the son's wife is engaged in the labour market and if the son's parents come from a poor economic background. Only individuals that are heads of households and earn a positive labor income are considered. Both subsamples of PSID (SRC and SEO) are considered, and probability weights are employed. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

Lastly, column 6 estimates Equation 4.4 including individuals (sons) fixed effects. The estimated coefficients of β are somewhat smaller, but the difference in the intergenerational elasticities of families facing a tax reform (in the example mentioned above) is similar, at around 4.2 percentage points.

Overall, these results suggest that fathers who face a tax cut are less likely to pass on their income status to their sons. This is an issue of important policy relevance, since it gives fiscal policy room to enhance intergenerational mobility and, therefore, equality of opportunity.

What mechanism could potentially bring about these results? One could consider a two-generation model where parents invest in the human capital of their sons. In this framework, parents that are positively affected by a fiscal reform, are able to endow their sons with higher human capital, and thus, allowi them to move up in the income ladder.

This potential mechanism also relates to the literature that explores the effects of aggregate shocks on intergenerational mobility. Solon (2004) considers, from a theoretical point of view, the effect of structural changes (reforms that affect the return to human capital). Recently, Nybom and Stuhler (2014) develop a model where current intergenerational mobility depends on events that affected previous generations, and empirically explore the effects of a universal school reform in Sweden to corroborate the implications from the model.

4.5 Robustness

ă In this section I explore the robustness of the results to different sample specifications.

First, Table 4.4 shows the results of estimating Equation 4.4 using different age specifications. Columns 1-3 consider a sample of father-son pairs aged 30 to 50. This wide election of age potentially augments the risk of life-cycle biases as mentioned in Section 4.2, but at the same time increases the number of observations.

The first column estimates Equation 4.4 with the new sample. The results are very similar to those in equation in Tables 4.1 and 4.3: around a third of the differences in income in the fathers generation are transmitted to their sons (in the absence of tax policies). The difference in the IGE of s family with a father that faces a tax increase of 1,000 dollars and family that doesn't is slightly above 4 percentage points. Column 2 adds time dummies to this specification and finds little change, with an estimated β of 0.3 (standard error of 0.02). Column 3 adopts a fixed-effects specification and finds a slightly smaller intergenerational elasticity, although the effect of taxation on this parameter

Table 4.4: Robustness: Different Age Specifications

	(1) age 30-50	(2) age 30-50	(3) age 30-50	(4) age 40-50	(5) age 40-50	(6) age 40-50
$income^{father}$	0.340***	0.303***	0.163***	0.312***	0.320***	0.212**
	(0.02)	(0.02)	(0.02)	(0.04)	(0.05)	(0.10)
ΔTAX	-0.524***	-0.804***	-0.457***	-2.350*	-4.689***	-0.285
	(0.18)	(0.28)	(0.11)	(1.32)	(1.76)	(0.67)
$income^{father}$	0.043***	0.065***	0.037***	0.210*	0.406***	0.021
$\times\Delta TAX$	(0.01)	(0.02)	(0.01)	(0.12)	(0.16)	(0.06)
time dummies	NO	YES	NO	NO	YES	NO
fixed effects	NO	NO	YES	NO	NO	YES
N	8887	8887	8887	1776	1776	1776

Note: OLS estimations of Equation 4.4. Sample period from 1967 to 2012. The sample is restricted to fathers and sons aged 30-40 (columns 1-3) with a time gap between generations of 25 years, and fathers and sons aged 40-50 (columns 4-6) with a time gap between generations of 30 years. Only individuals that are heads of households and earn a positive labor income are considered. Both subsamples of PSID (SRC and SEO) are considered, and probability weights are employed. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

remains largely unchanged.

Columns 3-4 estimate Equation 4.4 for a sample of individuals aged 40-50. This allows to explore the results when observing income at a latter stage in their careers. However, the number of observations is greatly reduced, resulting in higher standard errors. Columns 3 and 4 estimate the benchmark equation without and with time dummies, respectively. The estimates of the IGE are roughly similar but the importance of fiscal policy changes on this elasticity is substantially higher. However, the coefficients on $TAX^t_{i,t}$ and its interaction with the father's income are associated with large standard errors (even 10 times more than other specifications), what results on these estimates being just borderline significant (at levels of 10%). The specification with fixed effects shows point estimates more similar to the benchmark result, although some coefficients suffer from lack of significance.

Table 4.5 explores the robustness of the results to other changes in the sample selection criteria. Columns 1 to 3, report results when only considering the core-SRC sample of the PSID (i.e. excluding the over-sampled low-income families). Column 1 replicates the benchmark specification with the new sample and finds largely similar results: an elasticity of around 0.30 (standard error of 0.02) and a significant effect of variations in the tax code on this parameter (the difference in IG of families affected by a 1.000-dollar tax reform is around 4 percentage points). The inclusion of time dummies (column 2) does not significantly affect these conclusions. When considering fixed effects (column 3), the estimated IGE is lower (and less significant) but the effect of tax policies on this elasticity is quantitatively similar to the rest of specifications.

Lastly, columns 4-6 of Table 4.5 include not only sons but also daughters in the intergenerational analysis. In the literature on intergenerational mobility, daughters have been omitted from the analysis due to differences in earnings dynamics compared to their male siblings (Solon (1992)). However Lee and Solon (2009) measure intergenerational mobility for both father-son and father-daughter pairs and find that the evolution of the IGE for both groups is not substantially different. Columns 4 and 5 shows the estimates of Equation 4.4 and a version that includes year fixed effects, respectively. The results are similar to specifications that only include sons. Point estimates of the IGE fall in in the 0.33-0.36 range and the effect of exogenous variations in taxes on the intergenerational elasticity is very similar to the benchmark results. A specification that includes fixed effects (column 6) finds a smaller IGE parameter, while the effect of the fiscal policies on this object remains qualitatively and quantitatively close.

To sum up, measuring income at different age intervals, constructing the sample without the SEO (low-income) families or including both sons and

Table 4.5: Robustness: Different Sample Specifications

	(1) core	(2) core	(3) core	(4) daughters	(5) daughters	(6) daughters
$income^{father}$	0.294***	0.274***	0.084**	0.364***	0.326***	0.090**
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)
ΔTAX	-0.481***	-0.863***	-0.475***	-0.753***	-0.860***	-0.524***
	(0.17)	(0.27)	(0.13)	(0.20)	(0.29)	(0.14)
$income^{father}$	0.039***	0.068***	0.037***	0.061***	0.068***	0.041***
$\times \Delta TAX$	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
time dummies	NO	YES	NO	NO	YES	NO
fixed effects	NO	NO	YES	NO	NO	YES
N	3129	3129	3129	5551	5551	5551

Note: OLS estimations of Equation 4.4. Sample period from 1967 to 2012. Columns 1-3 restrict the sample to fathers and sons aged 30-40 with a time gap between generations of 25 years from the core-SRC PSID subsample. Columns 4-6 considers fathers and both sons and daughters aged 30-50 (columns 4-6) with a time gap between generations of 30 years. Only individuals that are heads of households and earn a positive labor income are considered. Both subsamples of PSID (SRC and SEO) are considered, and probability weights are employed. Robust standard errors are reported in brackets. $*$ *pvalue* $p < 0.1$, $**$ *pvalue* $p < 0.05$, $***$ *pvalue* $p < 0.01$.

daughters, do not significantly alter the results. All specifications find that tax cuts that benefit the fathers, increase the degree of mobility of their sons, as measured by the persistence of the income status between both generations.

4.6 Conclusions

This chapter has explored the degree and evolution of the intergenerational mobility in the US and how fiscal policy may have affected it. I find that fathers that experience a tax reduction as a result of a fiscal reform, are less likely to pass on their income status to their sons.

This evidence is particularly relevant from a policy point of view. Reductions of income inequality and the enhancement of equality of opportunity rank high in the policy makers' priorities (see Krueger (2012)). These results recognise that fiscal policy has a role in affecting, to some extent, how much of the income differences are transmitted across generations.

However, important pieces of the transmission mechanism of fiscal policy and intergenerational mobility still need to be understood. In Section 4.4 I suggested that investment in human capital by families that find themselves better off after a tax reduction could be a force at play. To explore whether this mechanism has the potential to explain the results of this chapter, one could analyse how the changes in taxes affect the number of years of education or the probability of attending college.

Another avenue to explore is the potential asymmetries of the effect of taxation on the intergenerational mobility. It is plausible that credit constrained households do not behave in the same way as unconstrained households when they experience a decrease in tax liabilities. To analyse this issue, a nonlinear framework would be required.

Lastly, there are other potential economic shocks that can affect the transmission of income differences across generations. For example, deep technological changes, large devaluations, or structural reforms. These avenues are left for future research.

Chapter 5

Appendices

A. Chapter 2: Data

The following data are obtained from the BEA's NIPA tables (last revision on 20 December 2013)

- Output is Gross Domestic Product from Table 1.1.5 (line 1).
- Government Spending is Federal Government Consumption Expenditures and Gross Investment from Table 3.9.5 (line 9).
- Total Tax Revenues are Federal Current Tax Receipts from Table 3.2 (line 2) plus Contributions for Government Social Insurance from Table 3.2 (line 11) minus Taxes on Corporate Income taxes from Federal Reserve Banks from Table 3.2 (line 8).
- Consumption is Personal Consumption Expenditures from Table 1.1.5 (line 2).

All these variables are expressed in real terms, deflated by the GDP deflator from Table 1.1.9 (line 1), and in per capita terms (divided by the civilian population aged 16 or more from Francis and Ramey (2009)).

Data from other sources:

- Consumer Confidence Index. Source: Conference Board (obtained via Thomson Reuters Datastream).
- Index of Consumer Sentiment. Source: Survey of Consumers, Thomson Reuters/University of Michigan.
- Business Confidence Indicator (industrial confidence in the manufacturing sector). Source: OECD (obtained via Thomson Reuters Datastream).

- Consumer Price Index. Source: BLS.
- Expected Inflation. Median expected price change during the next 12 months. Source: Survey of Consumers, Thomson Reuters/University of Michigan.
- Interest Rates. 3-Month Treasury Bill (Secondary Market Rate). Source: Board of Governors of the Federal Reserve System.

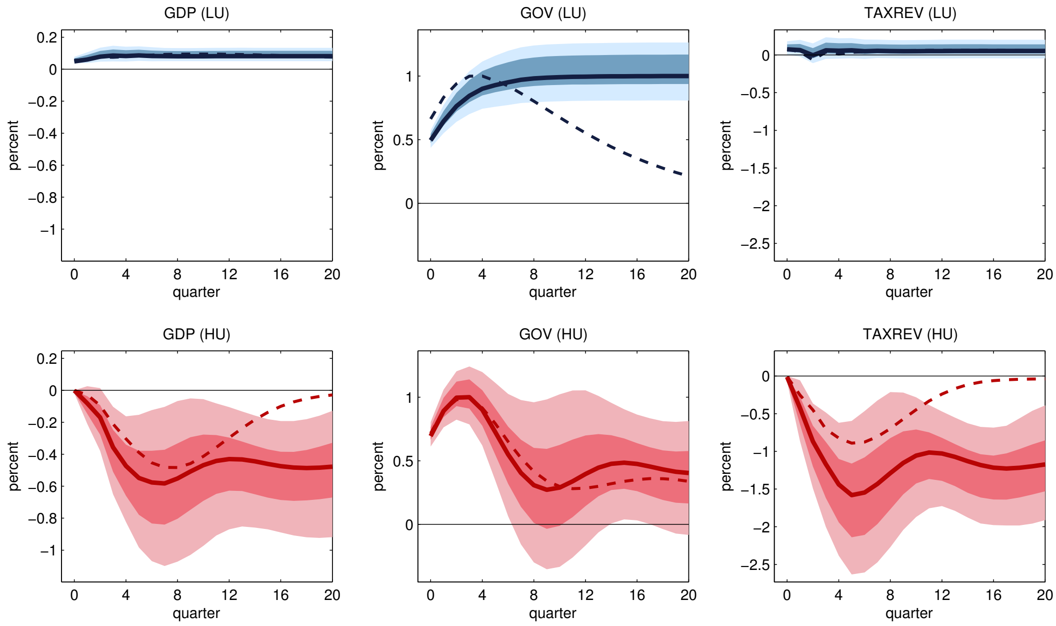
B. Chapter 2: Robustness to Trend Specifications

In Section 2.2, both equations 2.1 and 2.5 incorporate linear and quadratic trends, with the vector of variables $\boldsymbol{x}_t$ being in levels. In this subsection we reestimate these models, allowing for a stochastic trend by including the vector of variables in first differences (and omitting the deterministic trends).

Figure A1 compares the responses to government spending shocks identified with exclusion restrictions, in specifications with stochastic trends (solid lines) and with deterministic trends (dashed lines), during periods of HU and LU. In the latter case (LU), the only noticeable difference is the permanent effect of the shock on government spending when allowing for stochastic trends. During times of HU, output shows a very similar pattern in both specifications during the first two years. After that, the effect of the shock starts to disappear in the benchmark specification (dashed lines) while the effect is permanent in the stochastic trends specification.

Similar results are obtained when we consider the different effects in times of B and R (Figure A4). Interestingly, output remains positive and significant during times of B for the entire horizon, as opposed to what happens in the benchmark specification (where the effect of the shock lasts for about a year). When allowing for stochastic trends, the shock has permanent effects on gov-

Figure A1: Responses during HU and LU (SVAR identification, specification in first differences)

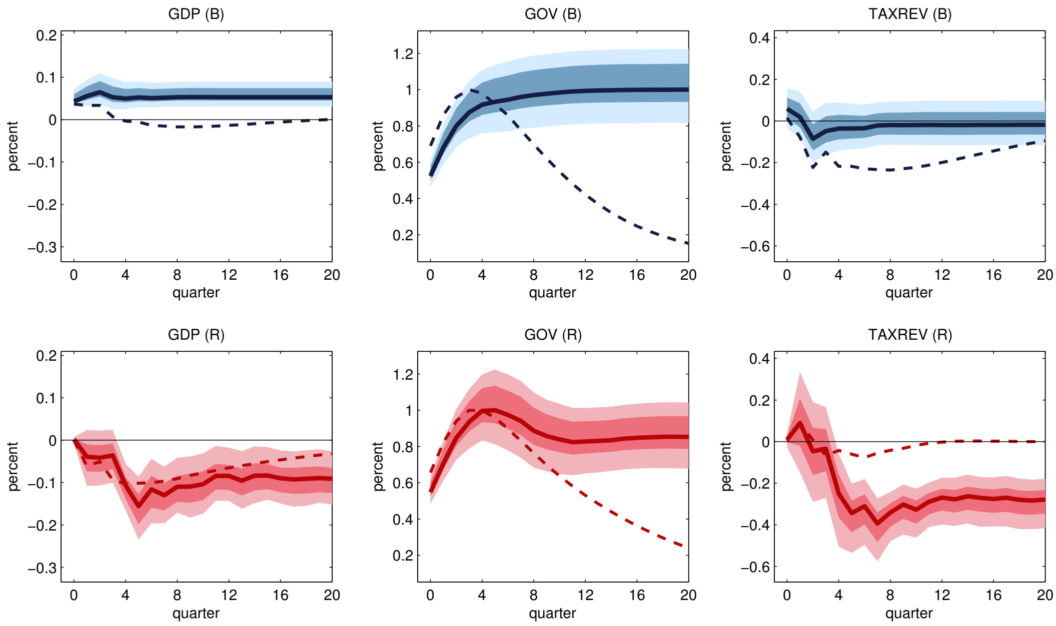


The top panel (in blue) shows responses to a government spending shock (identified using exclusion restrictions) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. The solid line plots the point estimates for the alternative specification using first differences. The dashed line plots the point estimates for the benchmark definition used in Section 2.2 with variables in levels. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

ernment spending during both R and B times. The responses of output during times of R are very similar in both trend specifications.

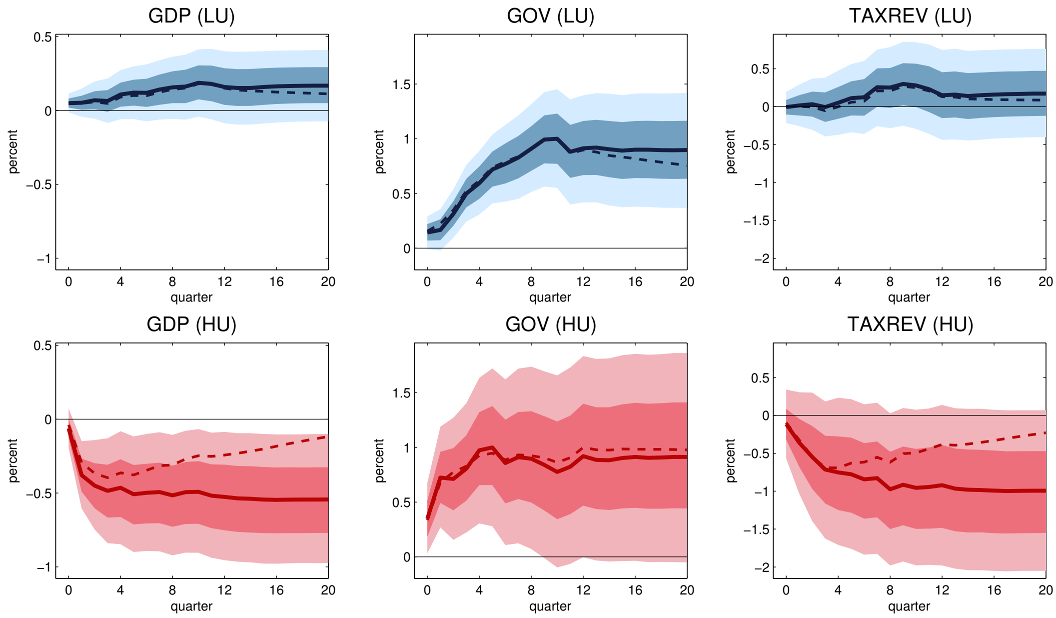
The same conclusions are reached when we repeat the analysis for the case of a government spending shock identified using narrative methods (equation 2.5). The responses during HU and LU (Figure A3) and during R and B (Figure A2) are fairly similar regardless of the assumption of deterministic or stochastic trends. During both R and HU, the response of output shows permanent effects when stochastic trends are considered, while the effects of the shock are temporary in the benchmark specification.

Figure A2: Responses during R and B (SVAR identification, specification in first differences)



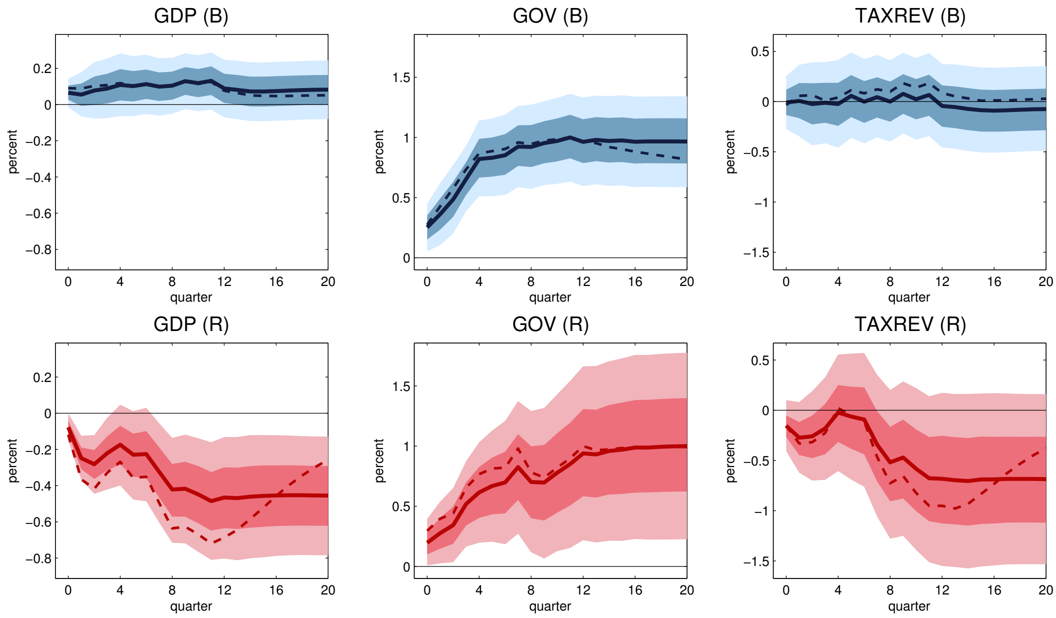
The top panel (in blue) shows responses to a government spending shock (identified using exclusion restrictions) during times of boom. The bottom panel (in red) shows responses during times of recession. The solid line plots the point estimates for the alternative specification using first differences. The dashed line plots the point estimates for the benchmark definition used in Section 2.2 with variables in levels. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

Figure A3: Responses during HU and LU (narrative identification, specification in first differences)



The top panel (in blue) shows responses to a government spending shock (identified from narrative accounts of defence spending) during times of low uncertainty. The bottom panel (in red) shows responses during times of high uncertainty. The solid line plots the point estimates for the alternative specification using first differences. The dashed line plots the point estimates for the benchmark definition used in Section 2.2 with variables in levels. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

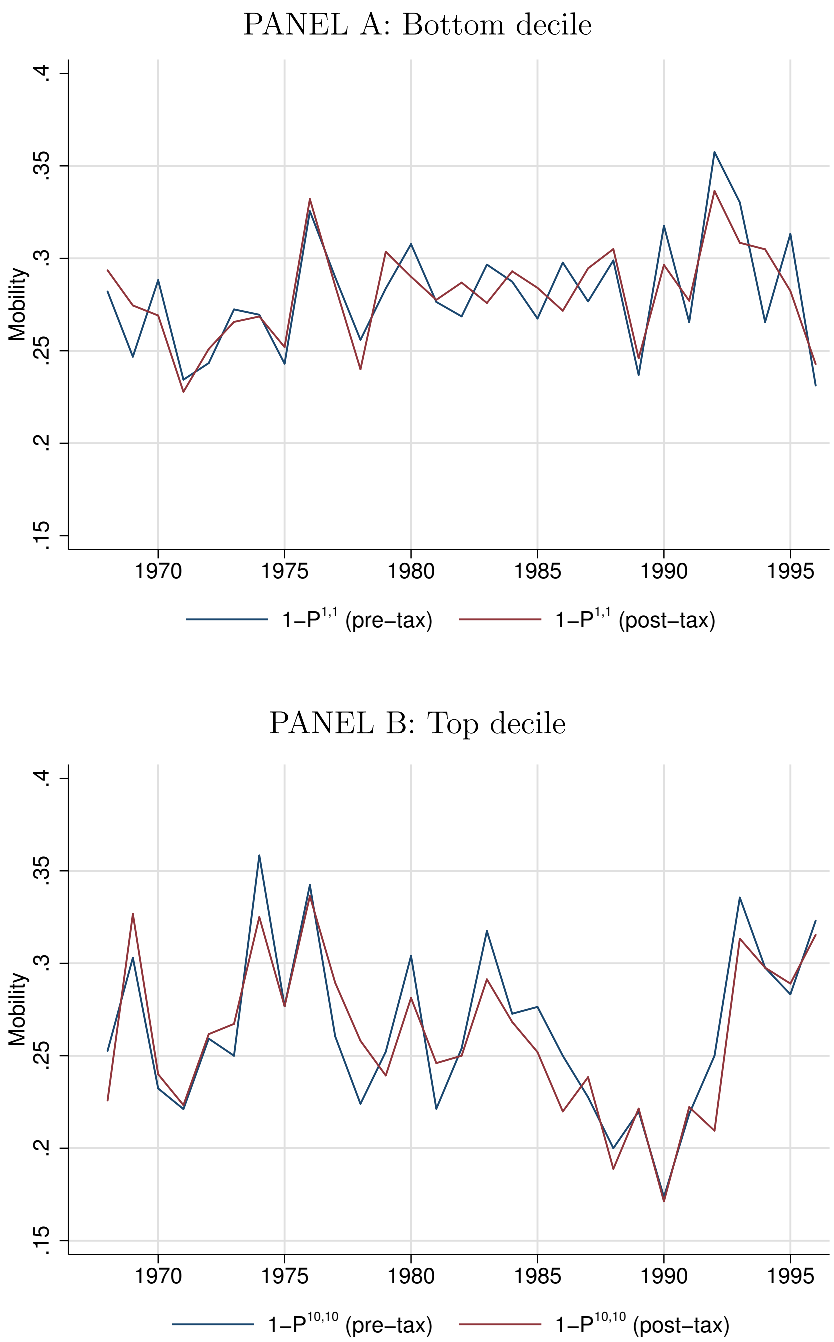
Figure A4: Responses during R and B (narrative identification, specification in first differences)



The top panel (in blue) shows responses to a government spending shock (identified from narrative accounts of defence spending) during times of boom. The bottom panel (in red) shows responses during times of recession. The solid line plots the point estimates for the alternative specification using first differences. The dashed line plots the point estimates for the benchmark definition used in Section 2.2 with variables in levels. The 68% and 95% confidence bands are computed using a non-parametric bootstrap.

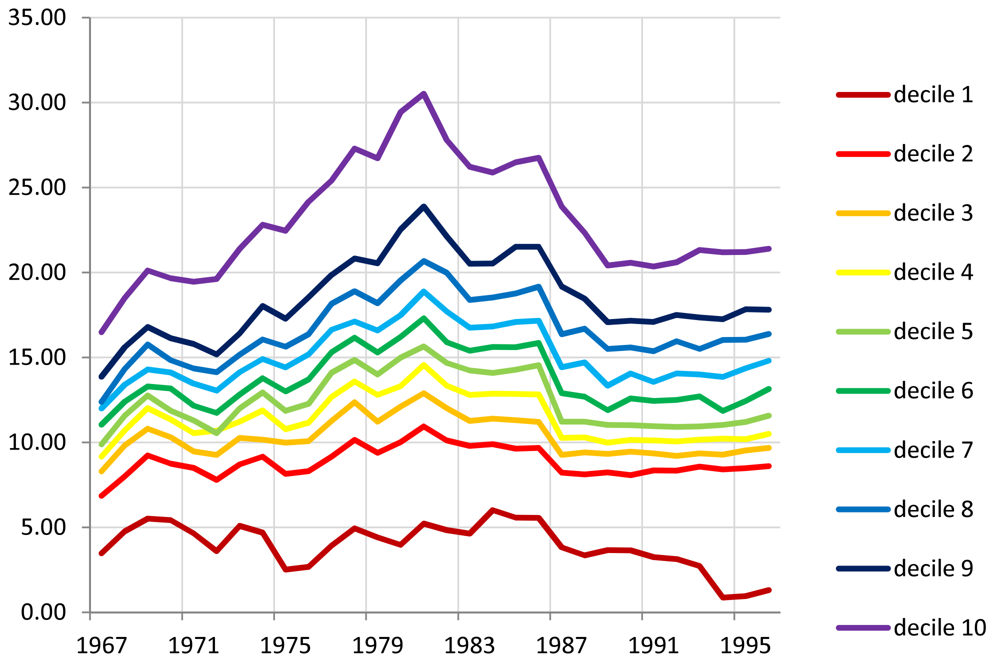
C. Chapter 3: Additional Figures and Tables

Figure A5: Evolution of the probability of transition matrix (1967-1996)



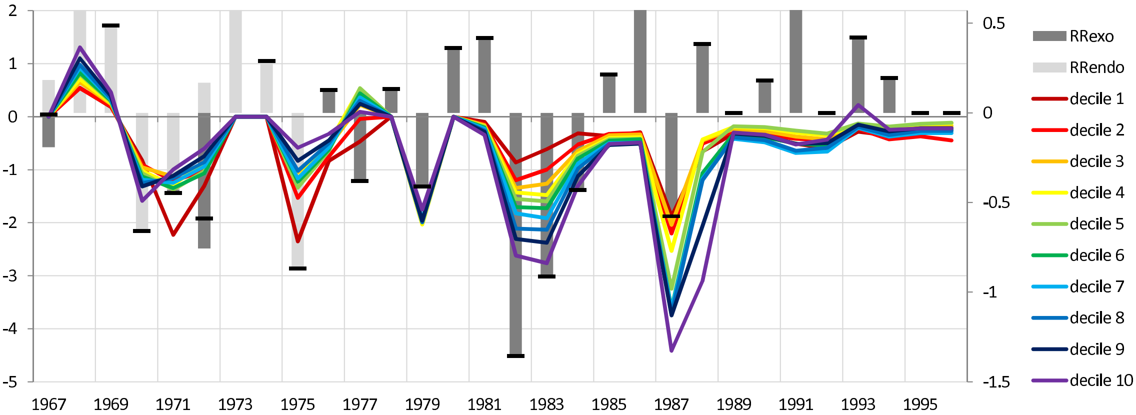
Note: Evolution of indices of mobility at the bottom and top deciles between 1967-1968. Panel A shows the evolution of the probability that a households leave the first decile of income (i.e. $1 - P^{1,1}$ in Equation 4.3). Panel B shows the evolution of the probability that a households moves down from the top decile of income (i.e. $1 - P^{10,10}$ in Equation 4.3). The distribution is computed using both pre-tax and post-tax income.

Figure A6: Variation in Average Tax Rates (1967-1996)



Note: Evolution of the average tax rates between 1967-1968.The figure displays the average ratio of total federal income tax liabilities to adjusted gross income (AGI) for each income decile. Tax liabilities are computed using TAXSIM and data from PSID.

Figure A7: Variations in Average Tax Rates due to legislated tax changes (1967-1996)



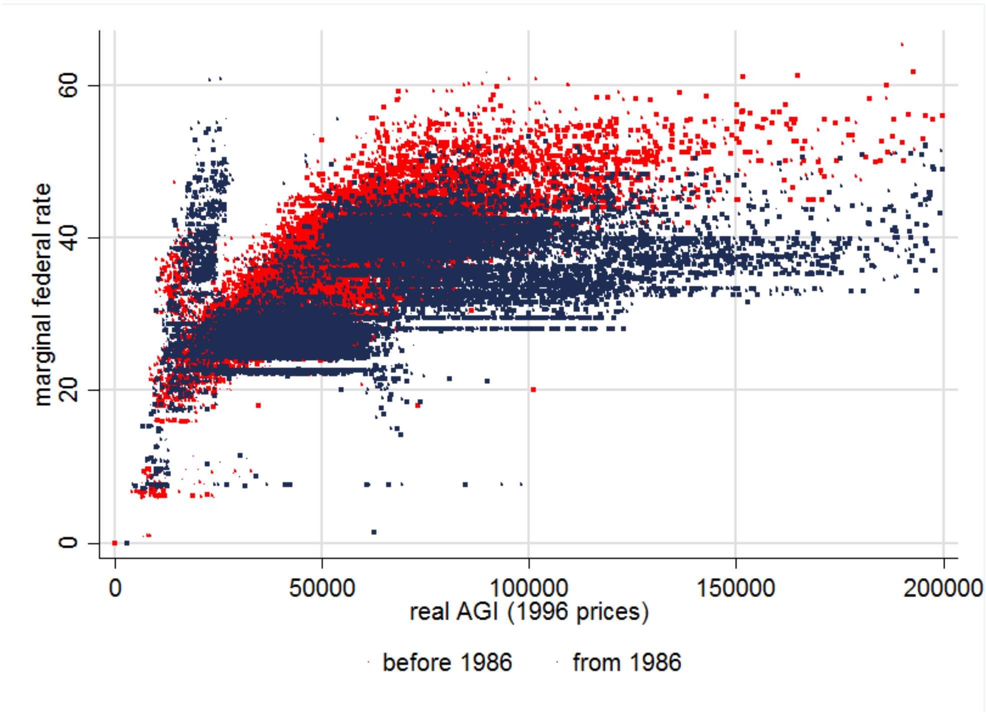
Note: Evolution between 1967 and 1996 of the instrument $\Delta\tau_{i,t}^{\bar{t}-1} = \tau_{i,t}^{t} - \tau_{i,t}^{t-1}$ (difference between the actual average tax rate and a counterfactual average tax rate computed using TAXSIM). Grey bars represent the narrative measure of legislated tax changes (as percentage of nominal GDP) from Romer and Romer (2010). These are classified as endogenous tax changes (related to the current state of the economy, in light grey) and exogenous tax changes (unrelated to the state of economy, in dark grey).

Table A1: Correlations between taxes (R&R, total) and mobility

	τ^{Romer}
move (P) pre-tax	-0.192
move (P) post-tax	-0.303
move (Q) pre-tax	-0.199
move (Q) post-tax	-0.263
trace (P) pre-tax	-0.112
trace (P) post-tax	-0.169
trace (Q) pre-tax	-0.173
trace (Q) pre-tax	-0.216
jump (pre-tax)	-0.206
jump (post-tax)	-0.225

Note: Correlation between indices of mobility and the narrative measure of total legislated tax changes (as percentage of nominal GDP) from Romer and Romer (2010). Mobility indices are the percentage of people changing income deciles (rows 1-2) or quintiles (rows 3-4), the normalised trace index (NTI, rows 5-6 for deciles and 7-8 for quintiles) and the average number of income deciles passed by a household between two adjacent years. The mobility indices are computed using both income before taxes (pre-tax) and after taxes (post-tax).

Figure A8: Variation in total marginal tax rates (1977-1996)



Note: Relationship between total marginal tax rates and real Adjusted Gross Income (1996 US dollars). Total marginal tax rates include the federal marginal rates on individual income, payroll and Social Security liabilities and State marginal tax rates for each household and year in the PSID before and after the 1986 tax reform (in red and blue, respectively).

Bibliography

Aaronson, D. and French, E. (2009). The Effects of Progressive Taxation on Labor Supply when Hours and Wages are Jointly Determined. *Journal of Human Resources*, 44(2):386–408.

Aaronson, D. and Mazumder, B. (2008). Intergenerational Economic Mobility in the United States, 1940 to 2000. *Journal of Human Resources*, 43(1):139–172.

Aastveit, K. A., Natvik, G. J., and Sola, S. (2013). Economic Uncertainty and the Effectiveness of Monetary Policy. *Unpublished manuscript, Norges Bank*.

Adermon, A., Lindahl, M., and Waldenström, D. (2015). Intergenerational Wealth Mobility and the Role of Inheritance: Evidence from Multiple Generations. Working paper, Uppsala University.

Aghion, P., Akcigit, U., Bergeaud, A., Blundell, R., and Hémous, D. (2015). Innovation and Top Income Inequality. Working paper, Harvard University.

Aiyagari, S. R. (1994). Uninsured Idiosyncratic Risk and Aggregate Saving. *Quarterly Journal of Economics*, 109(3):659–684.

Alesina, A. (2010). Fiscal Adjustments: Lessons from Recent History. In *ECOFIN meeting, Madrid, April*, volume 15.

Alesina, A. and Ardagna, S. (2013). The Design of Fiscal Adjustments. In *Tax Policy and the Economy, Volume 27*. University of Chicago Press.

Alloza, M. (2016). The Impact of Taxes on Income Mobility. Working paper, University College London.

Arrow, K. J. and Intriligator, M. D. (2015). Introduction to the Series. In Atkinson, A. B. and Bourguignon, F., editors, *Handbook of Income Distribution*, volume 2 of *Handbook of Income Distribution*, pages xvii –lxiii. Elsevier.

Atkinson, A. B. (1980). On Intergenerational Income Mobility in Britain. *Journal of Post Keynesian Economics*, 3(2):194–218.

Auerbach, A. and Gorodnichenko, Y. (2012). Measuring the Output Responses to Fiscal Policy. *American Economic Journal: Economic Policy*, 4(2):1–27.

Auerbach, A. J. and Gorodnichenko, Y. (2013). Fiscal Multipliers in Recession and Expansion. In Alesina, A. and Giavazzi, F., editors, *Fiscal Policy after the Financial Crisis*, pages 63–98. University of Chicago Press.

Bachmann, R. and Sims, E. R. (2012). Confidence and the Transmission of Government Spending Shocks. *Journal of Monetary Economics*, 59(3):235–249.

Baker, S., Bloom, N., and Davis, S. (2013). Measuring Economic Policy Uncertainty. Working Paper 13-02, Chicago Booth Research Paper.

Barro, R. J. and Redlick, C. J. (2011). Macroeconomic Effects from Government Purchases and Taxes. *Quarterly Journal of Economics*, 126(1):51–102.

Bartholomew, D. (1973). *Stochastic Models for Social Processes*. Wiley series in probability and mathematical statistics. J. Wiley.

Benhabib, J., Bisin, A., and Luo, M. (2015). Wealth Distribution and Social Mobility in the US: A Quantitative Approach. Working paper, National Bureau of Economic Research.

Bertola, G. and Drazen, A. (1993). Trigger Points and Budget Cuts: Explaining the Effects of Fiscal Austerity. *American Economic Review*, 83(1):11–26.

Bi, H., Leeper, E. M., and Leith, C. (2013). Uncertain Fiscal Consolidations. *The Economic Journal*, 123(566):F31–F63.

Black, S. E. and Devereux, P. J. (2011). Recent Developments in Intergenerational Mobility. *Handbook of Labor Economics*, 4:1487–1541.

Blanchard, O. and Perotti, R. (2002). An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output. *Quarterly Journal of Economics*, 117(4):1329–1368.

Bloom, N. (2009). The Impact of Uncertainty Shocks. *Econometrica*, 77(3):623–685.

Bloom, N. (2014). Fluctuations in Uncertainty. *The Journal of Economic Perspectives*, 28(2):153–175.

Bloom, N., Bond, S., and Reenen, J. V. (2007). Uncertainty and Investment Dynamics. *Review of Economic Studies*, 74(2):391–415.

Bloom, N., Floetotto, M., Jaimovich, N., Saporta-Eksten, I., and Terry, S. J. (2012). Really Uncertain Business Cycles. NBER Working Papers 18245, National Bureau of Economic Research, Inc.

Blundell, R., Duncan, A., and Meghir, C. (1998). Estimating Labor Supply Responses Using Tax Reforms. *Econometrica*, 66(4):827–861.

Bognanni, M. (2012). An Empirical Analysis of Time-Varying Fiscal Multipliers. Working paper, Working Paper. University of Pennsylvania.

Bradbury, K. L. (2011). Trends in US Family Income Mobility, 1969-2006. Working Papers 11-10, Federal Reserve Bank of Boston.

Brückner, M. and Tuladhar, A. (2013). Local Government Spending Multipliers and Financial Distress: Evidence from Japanese Prefectures. *The Economic Journal*.

Burnside, C., Eichenbaum, M., and Fisher, J. (2004). Fiscal Shocks and their Consequences. *Journal of Economic Theory*, 115:89–117.

Butrica, B. A. and Burkhauser, R. V. (1997). Estimating Federal Income Tax Burdens for Panel Study of Income Dynamics (PSID) Families Using the National Bureau of Economic Research TAXSIM Model. *Syracuse University Center for Policy Research Aging Studies Program Paper*, 12.

Caggiano, G., Castelnuovo, E., Colombo, V., and Nodari, G. (2015). Estimating Fiscal Multipliers: News From A Non-linear World. *The Economic Journal*, 125(584):746–776.

Chetty, R. and Hendren, N. (2015). The Impacts of Neighborhoods on Intergenerational Mobility: Childhood Exposure Effects and County-Level Estimates. Working paper, Harvard University.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The Effects of Exposure to better Neighborhoods on children: New evidence from the Moving to Opportunity experiment. *The American Economic Review*, 106(4):855–902.

Chetty, R., Hendren, N., Kline, P., and Saez, E. (2014a). Where is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States. *Quarterly Journal of Economics*, 129(4):1553–1623.

Chetty, R., Hendren, N., Kline, P., Saez, E., and Turner, N. (2014b). Is the United States still a Land of Opportunity? Recent Trends in Intergenerational Mobility. *The American Economic Review*, 104(5):141–147.

Christiano, L., Eichenbaum, M., and Rebelo, S. (2011). When Is the Government Spending Multiplier Large? *Journal of Political Economy*, 119(1):pp. 78–121.

Corsetti, G., Kuester, K., Meier, A., and Müller, G. J. (2013). Sovereign Risk, Fiscal Policy, and Macroeconomic Stability. *The Economic Journal*, 123(566):F99–F132.

Council of Economic Advisers (2015). Economic Report of the President. *USA Government Printing Office, Washington*.

De Nardi, M. (2015). Quantitative Models of Wealth Inequality: A Survey. Working Paper 21106, National Bureau of Economic Research.

Diamond, P. and Saez, E. (2011). The Case for a Progressive Tax: From Basic Research to Policy Recommendations. *Journal of Economic Perspectives*, 25(4):165–190.

Eide, E. R. and Showalter, M. H. (1999). Factors Affecting the Transmission of Earnings Across Generations: A Quantile Regression Approach. *Journal of Human Resources*, pages 253–267.

Fairlie, R. W. and Krashinsky, H. A. (2012). Liquidity Constraints, Household Wealth, and Entrepreneurship Revisited. *Review of Income and Wealth*, 58(2):279–306.

Fazzari, S. M., Morley, J., and Panovska, I. (2012). State-Dependent Effects of Fiscal Policy. *Australian School of Business Research Paper*, 27.

Feenberg, D. and Coutts, E. (1993). An Introduction to the TAXSIM Model. *Journal of Policy Analysis and Management*, 12(1):189–194.

Fernández-Villaverde, J., Gordon, G., Guerrón-Quintana, P. A., and Rubio-Ramírez, J. (2012). Nonlinear Adventures at the Zero Lower Bound. Working paper, National Bureau of Economic Research.

Fernández-Villaverde, J., Guerrón-Quintana, P. A., Kuester, K., and Rubio-Ramírez, J. (2011). Fiscal Volatility Shocks and Economic Activity. Working paper, National Bureau of Economic Research.

Fields, G. and Ok, E. (1999). The Measurement of Income Mobility: An Introduction to the Literature. In Silber, J., editor, *Handbook of Income Inequality Measurement*, volume 71 of *Recent Economic Thought Series*, pages 557–598. Springer Netherlands.

Francis, N. and Ramey, V. A. (2009). Measures of per Capita Hours and Their Implications for the Technology-Hours Debate. *Journal of Money, Credit and Banking*, 41(6):1071–1097.

French, E. (2005). The Effects of Health, Wealth, and Wages on Labour Supply and Retirement Behaviour. *Review of Economic Studies*, 72(2):395–427.

Giavazzi, F. and Pagano, M. (1990). Can Severe Fiscal Contractions be Expansionary? Tales of Two Small European Countries. In *NBER Macroeconomics Annual 1990, Volume 5*, pages 75–122. MIT Press.

Gittleman, M. and Joyce, M. (1999). Have Family Income Mobility Patterns Changed? *Demography*, 36(3):299–314.

Gonçalves, S. and Kilian, L. (2004). Bootstrapping Autoregressions with Conditional Heteroskedasticity of Unknown Form. *Journal of Econometrics*, 123(1):89–120.

Gottschalk, P. (1997). Inequality, Income Growth, and Mobility: The Basic Facts. *Journal of Economic Perspectives*, 11(2):21–40.

Grawe, N. D. (2006). Lifecycle Bias in Estimates of Intergenerational Earnings Persistence. *Labour economics*, 13(5):551–570.

Gruber, J. and Saez, E. (2002). The Elasticity of Taxable Income: Evidence and Implications. *Journal of Public Economics*, 84(1):1–32.

Haider, S. and Solon, G. (2006). Life-Cycle Variation in the Association between Current and Lifetime Earnings. *American Economic Review*, 96(4):1308–1320.

Hall, R. E. (2010). By How Much Does GDP Rise If the Government Buys More Output? *Brookings Papers on Economic Activity: Fall 2009*, page 183.

Hart, P. (1976). The Comparative Statics and Dynamics of Income Distributions. *Journal of the Royal Statistical Society. Series A*, 139(1):108–125.

Hassler, J. and Mora, J. V. R. (2000). Intelligence, Social Mobility, and Growth. *American Economic Review*, pages 888–908.

Heathcote, J., Storesletten, K., and Violante, G. L. (2009). Quantitative Macroeconomics with Heterogeneous Households. *Annual Review of Economics*, 1(1):319–354.

Hungerford, T. L. (1993). US Income Mobility in the Seventies and Eighties. *Review of Income and Wealth*, 39(4):403–417.

Hurst, E. and Lusardi, A. (2004). Liquidity Constraints, Household Wealth, and Entrepreneurship. *Journal of Political Economy*, 112(2):319–347.

Jäntti, M. and Jenkins, S. P. (2015). Income Mobility. In Atkinson, A. B. and Bourguignon, F., editors, *Handbook of Income Distribution*, volume 2 of *Handbook of Income Distribution*, pages 807 – 935. Elsevier.

Jordà, O. (2005). Estimation and Inference of Impulse Responses by Local Projections. *American economic review*, pages 161–182.

Keane, M. P. (2011). Labor Supply and Taxes: A Survey. *Journal of Economic Literature*, 49(4):961–1075.

Kopczuk, W., Saez, E., and Song, J. (2010). Earnings Inequality and Mobility in the United States: Evidence from Social Security Data Since 1937. *Quarterly Journal of Economics*, 125(1):91–128.

Krueger, A. (2012). The Rise and Consequences of Inequality in the United States. Delivered to the Center for American Progress, Jan 12.

Larrimore, J., Mortenson, J., Splinter, D., et al. (2015). Income and Earnings Mobility in US Tax Data. Working paper, Board of Governors of the Federal Reserve System (US).

Lee, C.-I. and Solon, G. (2009). Trends in Intergenerational Income Mobility. *The Review of Economics and Statistics*, 91(4):766–772.

Lerman, R. I. and Yitzhaki, S. (1995). Changing Ranks and the Inequality Impacts of Taxes and Transfers. *National Tax Journal*, 48(1):pp. 45–59.

Mertens, K. (2013). Marginal Tax Rates and Income: New Time Series Evidence. Working Paper 19171, National Bureau of Economic Research.

Mertens, K. and Ravn, M. O. (2011). Understanding the Aggregate Effects of Anticipated and Unanticipated Tax Policy Shocks. *Review of Economic Dynamics*, 14(1):27–54.

Mertens, K. and Ravn, M. O. (2012). Empirical Evidence on the Aggregate Effects of Anticipated and Unanticipated US Tax Policy Shocks. *American Economic Journal: Economic Policy*, 4(2):145–81.

Mertens, K. and Ravn, M. O. (2013). The Dynamic Effects of Personal and Corporate Income Tax Changes in the United States. *American Economic Review*, 103(4):1212–47.

Mertens, K. and Ravn, M. O. (2014). Fiscal Policy in an Expectations-Driven Liquidity Trap. *Review of Economic Studies*, 81(4):1636–1667.

Michaillat, P. (2014). A Theory of Countercyclical Government Multiplier. *American Economic Journal: Macroeconomics*, 6(1).

Mittnik, S. and Semmler, W. (2012). Regime Dependence of the Fiscal Multiplier. *Journal of Economic Behavior & Organization*, 83(3):502–522.

Mountford, A. and Uhlig, H. (2009). What Are the Effects of Fiscal Policy Shocks? *Journal of Applied Econometrics*, 24(6):960–992.

Nybom, M. and Stuhler, J. (2014). Interpreting Trends in Intergenerational Mobility. Working paper, Universidad Carlos III.

Nybom, M. and Stuhler, J. (2016). Heterogeneous Income Profiles and Lifecycle Bias in Intergenerational Mobility Estimation. *Journal of Human Resources*, 51(1):239–268.

Owyang, M. T., Ramey, V. A., and Zubairy, S. (2013). Are Government Spending Multipliers Greater during Periods of Slack? Evidence from Twentieth-Century Historical Data. *American Economic Review*, 103(3):129–34.

Parker, J. A. (2011). On Measuring the Effects of Fiscal Policy in Recessions. *The Journal of Economic Literature*, 49(3):703–18.

Perotti, R. (2004). Estimating the Effects of Fiscal Policy in OECD Countries. *IGIER Working Paper No. 276.*

Piketty, T. (2000). Theories of Persistent Inequality and Intergenerational Mobility. *Handbook of Income Distribution*, 1:429–476.

Piketty, T. (2014). *Capital in the Twenty-First Century.* Harvard University Press.

Piketty, T. and Saez, E. (2003). Income Inequality in the United States, 1913–1998. *Quarterly Journal of Economics*, 118(1):1–41.

Piketty, T. and Saez, E. (2007). How Progressive is the US Federal Tax System? A Historical and International Perspective. *Journal of Economic Perspectives*, 21(1):3–24.

Piketty, T. and Zucman, G. (2015). Chapter 15 - Wealth and Inheritance in the Long Run. In Atkinson, A. B. and Bourguignon, F., editors, *Handbook of Income Distribution*, volume 2 of *Handbook of Income Distribution*, pages 1303 – 1368. Elsevier.

Quadrini, V. and Ríos-Rull, J.-V. (2015). Inequality in Macroeconomics. In Atkinson, A. B. and Bourguignon, F., editors, *Handbook of Income Distribution*, volume 2 of *Handbook of Income Distribution*, pages 1229 – 1302. Elsevier.

Ramey, V. (2011a). Identifying Government Spending Shocks: It's All in the Timing. *Quarterly Journal of Economics*, 126(1):1.

Ramey, V. and Shapiro, M. (1998). Costly Capital Reallocation and the Effects of Government Spending. In *Carnegie-Rochester Conference Series on Public Policy*, volume 48, pages 145–194. Elsevier.

Ramey, V. A. (2011b). Can Government Purchases Stimulate the Economy? *Journal of Economic Literature*, 49(3):673–685.

Ramey, V. A. and Zubairy, S. (2014). Government Spending Multipliers in Good Times and in Bad: Evidence from US Historical data. Working paper, National Bureau of Economic Research.

Romer, C. and Romer, D. (2010). The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks. *American Economic Review*, 100(3):763–801.

Saez, E., Slemrod, J., and Giertz, S. H. (2012). The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review. *Journal of Economic Literature*, pages 3–50.

Saez, E. and Zucman, G. (2014). Wealth Inequality in the United States since 1913: Evidence from Capitalized Income Tax Data. Working Paper 20625, National Bureau of Economic Research.

Shorrocks, A. (1978a). Income Inequality and Income Mobility. *Journal of Economic Theory*, 19(2):376 – 393.

Shorrocks, A. F. (1978b). The Measurement of Mobility. *Econometrica*, 46(5):1013–1024.

Slemrod, J. (1996). High-Income Families and the Tax Changes of the 1980s: The Anatomy of Behavioral Response. In Feldstein, M. and Poterba, J., editors, *Empirical foundations of household taxation*, pages 169–192. University of Chicago Press.

Solon, G. (1989). Biases in the Estimation of Intergenerational Earnings Correlations. *Review of Economics and Statistics*, 71(1):172–174.

Solon, G. (1992). Intergenerational Income Mobility in the United States. *American Economic Review*, pages 393–408.

Solon, G. (2004). A model of intergenerational Mobility Variation over Time and Place. *Generational Income Mobility in North America and Europe*, pages 38–47.

Stiglitz, J. (2012). *The price of inequality*. Penguin UK.

Zidar, O. M. (2015). Tax Cuts For Whom? Heterogeneous Effects of Income Tax Changes on Growth and Employment. Working Paper 21035, National Bureau of Economic Research.

Zimmerman, D. J. (1992). Regression toward Mediocrity in Economic Stature. *American Economic Review*, pages 409–429.